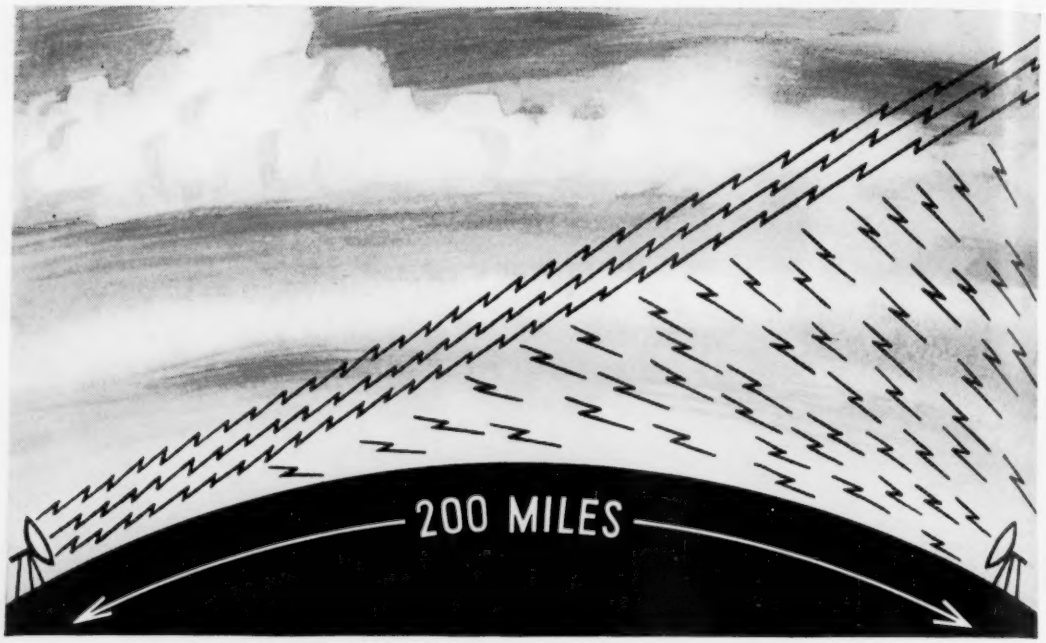
The SCIENTIFIC MONTHLY

DES
UNIVERSITY OF MICHIGAN
OCT 3 - 1955
PERIODICAL READING ROOM

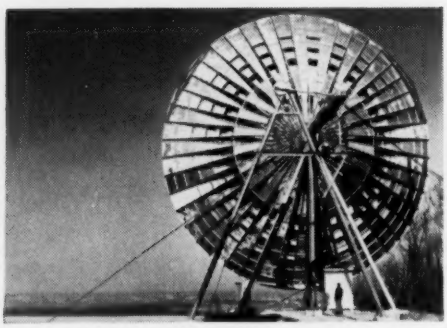
VOL. 81 NO. 4

OCTOBER 1955



Highly schematic drawing shows possible distribution of energy in ultra-high-frequency "over-the-horizon" transmission. Effect is similar to that of a powerful searchlight whose beam points into the sky. Light can be seen miles away from behind a hill even when searchlight lens is invisible.

SOMETHING NEW ON THE TELEPHONE HORIZON



This experimental 60-foot antenna (rear view) photographed at the Bell Telephone Laboratories in Holmdel, New Jersey, is designed for study of "over-the-horizon" phenomena.

BELL TELEPHONE LABORATORIES

Improving telephone service for America provides careers for creative men in scientific and technical fields.

Telephone conversations and television pictures can now travel by ultra-high-frequency radio waves far beyond the horizon. This was recently demonstrated by Bell Telephone Laboratories and Massachusetts Institute of Technology scientists using "over-the-horizon" wave propagation, an important recent development in the radio transmission field.

This technique makes possible 200-mile spans from station to station, instead of the 30-mile paths used for present line-of-sight transmission. It opens the way to ultra-high frequencies across water or over rugged terrain, where relay stations would be difficult to build.

In standard microwave line-of-sight transmission the stations are so spaced that the main beam can be used. But some signals drop off this main beam as it shoots outward into space. They reach distant points beyond the horizon after reflection or scattering by the atmosphere. The greater power and larger antennas of the "over-the-horizon" system permit recapture of some of these tiny signals and make them useful carriers.

Saunders Books

New (4th) Edition! . . . . Marshall-Lazier's Human Anatomy

The facts of anatomy are clearly presented with brief accounts of functional activities of different organs. The *New (4th) Edition* features a new discussion on the neurosensory system combining the sections on the organs of common sensation with the nervous system. The chapter on the endocrine system has been completely revised. 25 new illustrations have been added.

By Clyde Marshall, M.D., formerly Assistant Professor of Anatomy, Yale University School of Medicine. Revised by Edgar L. Lazier, Ph.D., Professor of Zoology, University of California at Los Angeles. 420 pages, 5½" x 8½", with 329 illustrations, 15 in color. $4.50. *New (4th) Edition!*

Romer's The Vertebrate Body

Here is one of the finest texts for a background course in comparative anatomy. A study of the form and function of the vertebrate body is interwoven with embryological, paleontological, histological and evolutionary material. Organs and systems of vertebrates are compared without over-emphasizing the human structure. Two handy appendices explain vertebrate classification and scientific terminology.

By Alfred S. Romer, Alexander Agassiz Professor of Zoology, Director, Museum of Comparative Zoology, Harvard University. 644 pages, 6⅛" x 9¼", with 390 illustrations. $7.00. *Second Edition!*

Villee's Biology

This is a general biology text written to interest the beginning student. Just the material he needs and can comprehend is presented, logically built on a factual basis. Emphasis is placed on the comparative and evolutionary aspects of biology. The text covers cell structure, plants, animals, systems of the human body, heredity, ecology and evolution—excellently illuminated by a total of 293 illustrations. Many teachers consider this text the most easily taught of its kind.

By Claude A. Villee, Harvard University. 670 pages, 6" x 9", with 293 illustrations. $6.50. *Second Edition!*

Willier, Weiss and Hamburger's Analysis of Development

This collaborative work is a modern synthesis of our knowledge of the principles and mechanisms of animal growth and development. Each contributor has given an authoritative presentation of the general problems, concepts and lines of investigation of his special topic and illustrated them with selected examples from experimental data. Tabulations and graphs are used wherever possible.

By Benjamin H. Willier, Professor of Zoology, Johns Hopkins University; Paul A. Weiss, Member, Rockefeller Institute for Medical Research, New York; and Viktor Hamburger, Professor of Zoology, Washington University, St. Louis. 735 pages, 7" x 10", with 248 illustrations. $15.00.

W. B. SAUNDERS COMPANY
West Washington Square Philadelphia 5

For Immediate Shipment

ALL Chemicals
ALL Drugs
ALL Reagents
ALL Pharmaceuticals

Ask for our new complete catalog

BIOS *Laboratories, Inc.*
17 West 60th St. New York 23, N. Y.
PLaza 7-8171

RESEARCH BIOCHEMICALS
OVER 1500
NUTRITIONAL BIOCHEMICALS Corp.
Cleveland 28, Ohio
Write for your copy—free catalogue—Dept. 103

---

technicon MOUNTING MEDIUM
the choice coverglass adhesive with *higher refractive index* • *hardens quickly*
Sample? . . . Of course, just write
TECHNICON CHEMICAL COMPANY, INC.
CHAUNCEY • NEW YORK

Looking for a PUBLISHER?

Do you have a book-length manuscript you would like published? Learn about our unusual plan whereby your book can be published, promoted and distributed on a professional basis. We consider all types of work, such as fiction, biography, poetry, scholarly and religious books, etc. New authors welcome. For more information, write for value booklet SM. It's free.

Vantage Press, Inc. 120 W. 31 St., N. Y. 1
In Calif.: 6253 Hollywood Blvd., Hollywood 28.

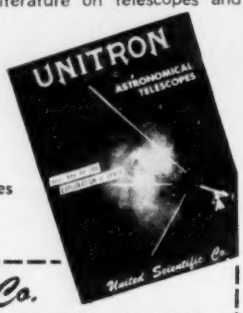
Get this FREE Buyer's Kit on UNITRON Astronomical Telescopes!

learn how to choose the best telescope for you

You want your telescope to be worthy of your investment and of the time you will devote to using it. UNITRON'S free "Buyer's Kit" is required reading for prospective telescope owners. Over 25 pieces of attractive, illustrated literature on telescopes and observing is yours for the asking.

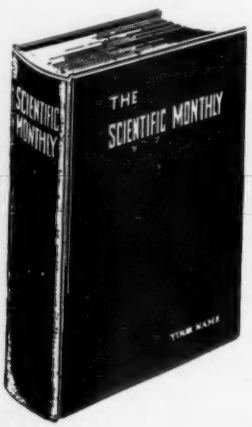
Learn about—

- equatorial and altazimuth mountings
- objective lenses
- refractors and reflectors
- magnification and eyepieces
- accessories



United Scientific Co.
204-6 MILK STREET • BOSTON 9, MASS.
Please rush to me, free of charge, your "Buyer's Kit" on Unitron Astronomical Telescopes.

Name .....
Street .....
City ..... State .....



Preserve your SM copies for quick, easy reference with attractive

<< BINDERS

$3 each; add 50¢ postage for orders outside U.S.A. (Name, 75¢ extra; year, 50¢ extra.) Personal check or money order, please!

The SCIENTIFIC MONTHLY 1515 Mass. Ave., NW Washington 5, D. C.

THE SCIENTIFIC MONTHLY

VOL. 81 OCTOBER 1955 NO. 4

Equal Temperament and the Thirty-one-Keyed Organ . . . . . *A. D. Fokker* 161

Soviet Cosmology and Communist Ideology . . . . . *Maxim W. Mikulak* 167

Changing Place of Soils in Agricultural Production . . . . . *Roy W. Simonson* 173

Are There Rules for Writing History of Chemistry? . . . . . *Aaron J. Ihde* 183

Mendel and the Rediscovery of His Work . . . . . *Edward O. Dodson* 187

How Does Rock Break? . . . . . *Fred C. Bond* 196

Book Reviews of *Cultural Anthropology; The Evolution of an Insect Society; Readings in Anthropology; Animal Life in Deserts; Evolution of the Vertebrates; Gundriss der Allgemeinen Zoologie; Introduction to Physical Geology; Two Years in the Antarctic; Science Reasoning and Understanding; The Chemistry of Living Cells; Botany Principles and Problems; The Elements of Chromatography; Soil Fertility; Minerals in World Industry; Experiments in Organic Chemistry; The New Warfare; Applied Entomology; Physics: a Descriptive Interpretation*; Books Reviewed in *Science*; and New Books 199

Letters from *Gerald C Helmstadter, David M. Pratt, James C. Crumbaugh, Sherman Ross,* and *Ray C. Hackman* 209

Cover: Mushroom Coral

[*Courtesy A. M. Winchester, Stetson University, see page 172*]

AAAS EDITORIAL BOARD

DAEL WOLFLE, *Acting Editor* CHARLOTTE V. MEETING, *Assistant Editor*

MARK H. ADAMS
WALLACE R. BRODE
BENTLEY GLASS
KARL LARK-HOROVITZ
EDWIN M. LERNER
WILLIAM L. STRAUS, JR.

Advertising Representative: EARL J. SCHERAGO

THE SCIENTIFIC MONTHLY, established in 1872 as *The Popular Science Monthly*, has been an official publication of the American Association for the Advancement of Science since 1915. It is published for the Association at Business Press, Inc., Lancaster, Pa.

Copyright, 1955, by the American Association for the Advancement of Science. Entered at the Lancaster, Pa., Post Office as second class matter under the act of March 3, 1879.

All correspondence should be addressed to THE SCIENTIFIC MONTHLY, 1515 Massachusetts Ave., N.W., Washington 5, D. C. Manuscripts should be typed with double spacing and submitted in duplicate. The AAAS assumes no responsibility for the safety of manuscripts and photographs or for the statements and opinions expressed by contributors.

Change of address: Four weeks are required to effect a change of address. If possible, please furnish an address stencil label from a recent issue. Be sure to give both old and new addresses, including postal zone numbers, if any.

Annual subscriptions: $7.50, domestic and foreign. Single copies, 75¢. Special rates to members of the AAAS.

THE SCIENTIFIC MONTHLY is indexed in the *Reader's Guide to Periodical Literature*.

The AAAS also publishes SCIENCE. Subscription and advertising rates will be furnished on request.

Science and Technology

(From the month's news releases; publication here does not constitute endorsement.)

Portable X-ray Unit

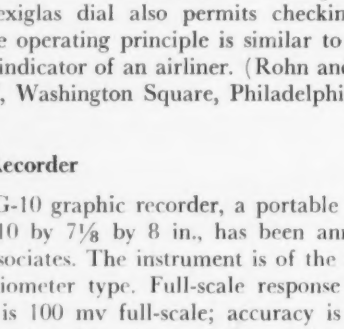
Three interlocking units form the dispensing, exposing, and development systems of a new portable x-ray device that is designed for use in the production of industrial radiographs. The unit may be used in conjunction with a portable x-ray generator to detect flaws in metal castings, girders, or construction welds. The film dispenser is loaded with a canister of 24 films that are transferred individually to a holder for exposure; exposed films can be unloaded directly into the processing unit for developing. According to the manufacturer, the system can be adapted for use with standard x-ray equipment; it can also be used with a radioactive source. The units, which are molded from Bakelite phenolic resins, weigh about 25 lb, including case, chemicals, and accessories. (Ind-X System, Inc., Dept. SM, 115 Belmont Ave. N., Seattle 2, Wash.)

Speedometer

An air speed indicator designed to be mounted on the hood of an automobile permits the driver to check his speed without taking his eyes from the road. Combined with an insect deflector, the transparent, green-tinted Plexiglas dial also permits checking speed at night. The operating principle is similar to that of the air speed indicator of an airliner. (Rohn and Haas Co., Dept. SM, Washington Square, Philadelphia 5, Pa.)

Graphic Recorder

Model G-10 graphic recorder, a portable model that measures 10 by 7⅛ by 8 in., has been announced by Varian Associates. The instrument is of the self-balancing potentiometer type. Full-scale response is 2.5 sec; sensitivity is 100 mv full-scale; accuracy is 1 percent; maximum allowable signal source resistance is 0.5 Mohm. The recorder is designed to be used directly as a recording millivoltmeter or, with appropriate transducers, as a means for recording pressure, light intensity, and temperature. (Varian Associates, Dept. SM, 611 Hansen Way, Palo Alto, Calif.)



Geiger Counter

Halotron "15" is a portable 15-tube geiger counter that is designed for use in detecting uranium ore and for general survey work. According to the manufacturer, the unit's sensitivity is greater than that of many geiger counters and its performance is comparable to that of some scintillation counters. The unit, which is waterproof and shockproof, is powered by two standard "D" flashlight cells and three miniature "B" batteries; its dimensions are 3¼ by 8 by 7⅞ in. and its weight is approximately 5 lb. Cover and case are made of drawn aluminum; interior metal parts are cadmium plated. (Nuclear Measurements Corp., Dept. SM, 2460 N. Arlington Ave., Indianapolis 18, Ind.)

Stereoscopic Camera

A new stereoscopic camera that is capable of taking three-dimensional photographs in color or black and white has been announced. Invented by David Donaldson of the Howe Laboratory of Opthalmology, Harvard Medical School, the camera is designed to eliminate the optical distortions inherent in ordinary stereo equipment. Bellows extension, inter-lens distance, and parallax correction scales are calibrated according to magnification. Once the appropriate magnification or minification has been selected, all scales are set to this number before the camera is focused. A variable intensity light source (electronic strobe flash) of short duration, powered by a separate amplifier, is also calibrated according to magnification. The instrument is designed so that the user can duplicate exposure, magnification, and stereoscopic effect. A built-in stop prevents double exposures; the lens openings remain constant at a small aperture. (Perkin-Elmer Corp., Dept. SM, Norwalk, Conn.)

Compass and Clinometer

Two new pocket-sized instruments, a compass and a clinometer, have been designed to replace the engineer's transit, Abney level, prisms, and hand level in preliminary surveying and reconnaissance work. Both instruments have been imported from Finland. The Suunto Oy optical-reading compass is calibrated in degrees; accuracy is ±10 min. The Suunto Oy optical-reading clinometer incorporates two optical scales, one calibrated in degrees and the other in percentages; accuracies are ±10 min and ±0.25 percent. Both instruments are of rugged one-piece construction. Scales are viewed through a self-focused, parallax-free lens. Moving parts operate on sapphire bearings. The damping liquid, which lubricates the bearings, does not freeze in the arctic or boil in the tropics. (Imperial Trading Co., Dept. SM, 1116½ N. Western Ave., Los Angeles 29, Calif.)

THE SCIENTIFIC MONTHLY

OCTOBER 1955

Equal Temperament and the Thirty-one-Keyed Organ

A. D. FOKKER

Dr. Fokker, a theoretical physicist, is known for his early work in quantum statistics and for the Fokker equation, which played an important role in the formulation of early quantum theory. He is now director of the physics department of Teylers Museum, Haarlem, Netherlands, where he developed the material that is described in this article.

THERE are a great many musics among the peoples inhabiting our planet, and they show differences as great as the languages spoken by the various races and nations. In addition, there is the music of the birds, and there are melodic lines of human speech. It is a very difficult task to represent all this music by written symbols. This task requires centuries of analyzing and systematizing work. Too soon one is eager to believe that one's own music is the most natural, and that one's own way of putting it on paper in notes on staves is the most straightforward and efficient way of recording it, and that there is no alternative. The fact is, however, that our subdivision of the interval of the octave into 12 equal semitones represents a solution for certain special needs only. It embodies a stage in the historical development of the art, a system with its virtues and its deficiencies, and one that is likely to be modified or even relinquished during the course of historical evolution. There are vast civilizations of autochtonic musics, not very remote from the idioms of Western civilization, that must remain behind the horizon of our understanding conscience, unless the system of 12 equal semitones in the octave is disposed of, and a better approximation is substituted for it.

The word *octave,* which denotes the fundamental distance (and similarity) between the notes sung by men and women simultaneously when they join in a common hymn or song, testifies to a specified musical tradition with scales in which the eighth note is identified with the first. The Greeks did not pin down music in one scale of seven (8 – 1 = 7). They called this relation *diapason,* that is "through all," and by so doing they put into evidence the idea that the whole variety of notes is contained in a sequence between the first and the last note of a diapason. Nowadays two notes separated by this interval are known to have frequencies in the relationship of 1:2, the higher note having double the frequency of the lower. Such notes, in spite of their differences, are identified as having the same musical value, nowadays as well as among the ancient Greeks.

In what follows I take for granted that the reader is acquainted with the names of the notes, A, B, C, D, E, F, G, A (or la, ti, do, re, mi, fa, so, la) and their lowering and raising by signs called flats (♭) and sharps (♯): A-flat = A♭, C-sharp = C♯, and so forth. Where necessary, I have added refinements.

Pythagoras is reported to have systematized the collection of notes by selecting a certain specified interval relationship and ordering a sequence of notes according to this relationship, with the re-

jection or exclusion of all notes not entering into this order. The selected relationship was the one that is presently called the *perfect fifth*, the notes in question having frequencies in the proportion 2:3. Thus the number 3 comes in (in spite of the historical name *fifth*). The series of notes arranged by this interval relationship was

F : C : G : D : A : E : B

The sequence may be continued to the right, where sharps come in:

E : B : F♯ : C♯ : G♯ . . . ,

or to the left, where flats come in:

. . . D♭ : A♭ : E♭ : B♭ : F : C

Obviously this was a one-dimensional series. The only true consonances to be found were in the octaves, in the perfect fifths and, in a lesser degree, in the fourths. The relationship C:E (in numbers 64:81) was considered to be a discord, and so it was in Pythagoras' linear system.

In the Renaissance period singers and composers of polyphonic music discovered that by a slight alteration of Pythagoras' E one arrived at a perfect consonance of C and E. This was the concord of notes with frequencies in the relationship 4:5, (64:80), where the number 5 comes in (the whim of history makes us call the interval 4:5 a *perfect third*!). Accordingly one is led to a two-dimensional specification of the legitimate notes with concordant relationships, as follows:

		C♯	G♯	D♯	A♯	E♯	B♯	
	D	A	E	B	F♯	C♯	G♯	D♯
E♭	B♭	F	C	G	D	A	E	
	G♭	D♭	A♭	E♭	B♭	F		

In this field one meets two kinds of semitones, major semitones—such as C♯ : D and D : E♭—and minor semitones—such as D : D♯, and E♭ : E. In the major semitones the frequency ratio is 15:16; in the minor semitones the ratio is 24:25.

Obviously this procedure leads to an innumerable crowd of notes; and, although in the human voice there is no restriction whatever, on an instrument it is not possible to provide separate strings, pipes, or keys for all of them. There is the slight difference of notes on different lines bearing the same name. In an early stage musicians agreed to discard this difference. In the 16th century Gioseffo Zarlino said that one should adjust the sequence of "fifths" in C : G : D : A : E in order to have a perfect concord between C and E. This is a very slight readjustment, perceptible to a very acute ear but not a disturbing nuisance.

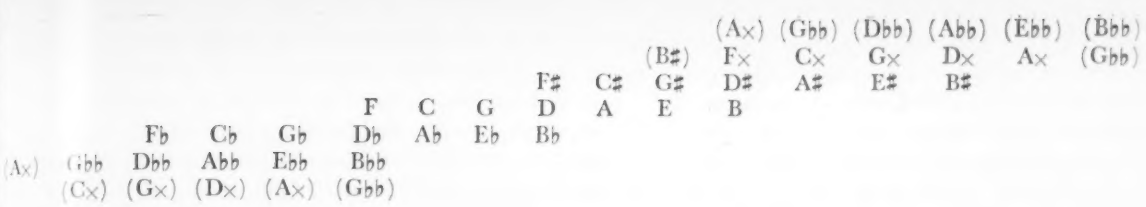
Rather bigger are the differences between notes such as D♭ and C♯ or G♯ and A♭, which are separated by three intervals in a vertical column. If one can afford only a string or a pipe for G♯, and not one for A♭, one can sound the perfect common chord E : G♯ : B but not the chord A♭ : C : E♭. The increasing demand that the chords be available in all keys led to a compromise. The present equal temperament is based on the decision to disregard these differences, thus identifying all notes bearing the same names and every fourth note in the vertical columns. Thus one is left with a collection of 12 notes: (Notes that duplicate notes already put down in other places are in parentheses.)

	(F)	(E♭)	(B♭)			
	(E)	B	F♯	C♯	G♯	(E♭)
(F)	C	G	D	A	E	(B)
	(G♯)	E♭	B♭	F	(C)	

All semitones are leveled, and the ratio is very approximately 17:18. This equal temperament was advocated by Vincenzio Galileo, in the 16th century. The exact figures to four decimal places were calculated by Simon Stevin early in the 17th century. Not before the middle of the 19th century was the equal temperament universally accepted.

No doubt this equal temperament with its perfect cyclic symmetry has rendered great services in the evolution of Western musical civilization. One must recognize this. On the other hand, many musicians are aware of the deficiency of the chords sounded on the pianoforte and the organ that have been tuned according to the duodecimal temperament. Their complaint is the same as the objection of the 17th- and 18th-century musicians who refused to have their beautiful harmonic consonances spoiled by such a compromise. One of them was Christian Huygens, who lived in the 17th century. He was a very fine musician, but he also happened to be a mathematical genius. As a mathematician he discovered a new harmonic cycle (*nouveau cycle harmonique*) when, in attempting to preserve the difference between major and minor semitones, he took just this difference as an elementary unit for the even division of the octave. This same unit is the excess of an octave over three perfect thirds. It is about half the minor semitone and one-third of the major semitone. Thus it is one-fifth of a whole tone, and Huygens called it *diesis*. It follows that the octave is partitioned into 31 elementary steps.

The choice of 31 notes to represent the boundless meshwork of notes related by the ratio numbers 2, 3, and 5 evidently means the adoption of another identification of notes instead of the identification of flats and sharps found in the black keys of the ordinary keyboard (C♯ and D♭, and so forth). Indeed, in Huygens' new cycle notes such as B-double-sharp and D-double-flat are taken to be identical. Other examples are found in A× and C♭♭, in E× and G♭♭, D× and F♭♭. The mesh in the net occupied by the 31 notes is as follows.



One finds the seven basic notes A, B, C, D, E, F, G, their flats and sharps, their double flats and double sharps, ruling out two double flats that are identical to two double sharps, and vice versa.

Such a multitude of notes was far too much for the comprehensive faculties of Huygens' contemporaries, and his discovery was left to oblivion. Yet, he made another very significant observation. He remarked that, besides a faithful rendering of the perfect third, his harmonic cycle also provided a nearly exact reproduction of the perfect seventh. The perfect seventh is the interval of the fourth and the seventh harmonic notes of a string. It is a concord in which the number 7 comes in. Huygens claimed that it was just as valid and legitimate as the other perfect intervals used in music and a beautiful concord, but contemporary public opinion was against him. This repudiation of the perfect seventh was not unlike the disqualification of the perfect third as a concord by medieval people.

In the field of notes related by the numbers 2, 3, and 5, one finds pairs—such as C and A-sharp—related by the numbers 128 ($= 2^7$) and 225 ($= 3^2 \times 5^2$). Owing to the complexity of the latter number, the interval might be taken to sound a discord. But next to 225 lies 224 ($2^5 \times 7$), and $128 : 224 = 4 : 7$. This simple ratio fully qualifies the interval as a concord, and Huygens' point was that if his cycle were adopted, the number 225 would be readjusted to a very near approach to 224. Huygens was aware of latent possilities and beauties. As an example he pointed out the beauty of the chord B♭ : E : G♯. This is a marvelous remark, and in his days it must have been very shocking indeed, because every 17th-century professional musician would have denounced the chord as utterly discordant.

Nowadays there is a new demand that the system underlying our music be improved. Well known is the attempt of Aloys Haba. He was aware that in order to reproduce native songs of his countrymen in Czechoslovakia he needed a finer grain, so to speak, and therefore he halved the equal semitones. He tried to use quarter-tones. However, it is clear that by simply adding 12 more notes between the existing 12 notes one cannot improve the harmonic relationships and the musical quality of the original 12 notes. What one needs for improvement is not quarter-tones but fifths of a tone—the system Huygens used.

Again, Béla Bartòk, the Hungarian composer who died in 1945 in the United States, recognizing the value of the Hungarian peasant music both as a living tradition and as a source of inspiration for modern music, stated that he over and over again met the interval of the harmonic seventh and that he therefore laid at the bottom, as a basic foundation for music, a chord of four notes, adding the perfect seventh to the common chord. Bartòk's testimony carries great weight, for he was one of the very greatest modern composers. Therefore, for the evolution of music, it is very urgent that we find an equal temperament suitable for the reproduction of Bartòk's fundamental chord, which may be specified by the harmonic numbers 4 : 5 : 6 : 7. The "tricesimoprimal" equal temperament discovered by Huygens 3 centuries ago fulfills the requirement.

The aim of an equal temperament is the reproduction of certain intervals, which are chosen because they are considered fundamental, as integer manifolds of one single unit. This means that the problem is, given the measures of the intervals as certain numbers, to find their common divisor. Of course, it is well known that the musical intervals are represented by irrational numbers and that such numbers can never have a common divisor. Thus we must try to find an approximate divisor. The octave (1 : 2) measures, by definition, $O = 1200$ cents (the cent is the hundredth part of an equal semitone). The perfect fifth (2 : 3) has $Q = 702$ cents. The perfect third has $T = 386$ cents, and the perfect seventh $S = 969$ cents. Any divisor common to two numbers is also a divisor of their difference. Now, looking at

$$\begin{aligned} O - 3T &= 42 = 1 \times 42 \\ 2T - Q &= 70 = 2 \times 35 \\ O - S &= 231 = 6 \times 38.5 \\ T &= 386 = 10 \times 38.6 \end{aligned}$$

one finds that T must have 10 units; $O = 31$ units; $Q = 18$ units; and $S = 25$ units. The result, $O = 31$ units, whence 1 unit = 38.7 cents, fully confirms the discovery of Huygens. It could not have failed to do so.

The question of how to handle the harmonic seventh in music has been raised several times. The use of it has been advocated by several authors, beginning as early as the middle of the 18th cen-

tury. Among them was Giuseppe Tartini in Padova, who allotted this note a place in the gamut. Accidentally, it happened that in those years the duodecimal temperament started its conquest of Western music. It could reproduce intervals of 900 and of 1000 cents, but not 969 cents, as required by the harmonic seventh. The duodecimal temperament therefore inhibited the use of Tartini's seventh. The tricesimoprimal temperament had never received any attention, and therefore Tartini's attempt was doomed to fail, and it also fell into oblivion. The time for recollection might now have come.

From the mathematical side, Leonhard Euler, a contemporary of Tartini—neither known to the other—showed another method for introducing the seventh into the structure of actual music. Euler was also unaware of Huygens and his harmonic cycle of 31.

Giuseppe Tartini left us a means for notation on the stave. He introduced half-flats and one-and-a-half-flats. The perfect seventh on G is slightly flatter than the discordant F, and we may call it F-minus, or F-half-flat. The perfect seventh on C is slightly flatter that B-flat, and we may call it B-flat-minus or B-one-and-a-half-flat. Tartini's sign for a half-flat is a hook pointing down, and his one-and-a-half-flat is a certain contraction of this hook with an ordinary flat (Fig 1). In the tricesimoprimal temperament, F-half-flat is identified with E-sharp. Musicians have a certain liking for connecting a relation of a sixth with the letters G and E. Therefore it might be safer to write F-minus instead of E-sharp. The same remark applies to the use of B-flat-minus rather than A-sharp if one meets the harmonic seventh related to C.

Obviously one could benefit by having a notation for a half-sharp too. The perfect seventh below C is a trifle sharper than D. Let us call it D-plus, or D-half-sharp. Again, the perfect seventh below B is a trifle sharper than C-sharp, call it C-sharp-plus, or C-one-and-a-half sharp. The sign for a sharp having two vertical strokes, it might be natural to write one-half sharp with a single vertical stroke, and, similarly, one-and-a-half sharp with three vertical strokes. The subharmonic primary chords with four notes would therefore show on the staves as they do in Fig 2. A sequence of notes proceeding by fifths of a tone is shown below in Fig. 3 twice, first with the available signs of Tartini, second with available sharps. At once the identity, in the tricesimoprimal temperament, of, for example, E-flat and D-sharp-plus or E-double-flat and D-one-half-sharp, is obvious.

I return to the two-dimensional representation of all the notes related by the numbers 3 and 5. If we now recognize the harmonic seventh as a legitimate concord, there is evidently good reason to extend the representation to a three-dimensional one. That will appear as a kind of three-dimensional chessboard. Crystallographers would speak of a crystal lattice with notes instead of atoms. In crystals we meet a certain periodicity of structure, and elementary cells are reproduced over and over again, so that not all atoms are different, and the variety is a finite variety only. In a similar way, by establishing identities between various notes, the threefold endless multitude of notes in the harmonic lattice is reduced to a finite set (Fig. 4).

In addition to the identification of notes in the two-dimensional harmonic plane of relationships made by threes and fives, there are identifications related to 7 and one or both of the others. We have already come across the identification of $225 = 3^2 \times 5^2$ and $224 = 2^5 \times 7$, that is, the identification of A-sharp and B-flat-minus relative to C. Another identification is reasonable, that of $343 = 7^3$ and $341\frac{1}{3} = 1024/3 = 2^{10}/3$. In relation to C, these notes come out as G-double-flat-minus and F. Three such identities, or—speaking geometrically, three identity vectors—delimit a unit cell in the boundless three-dimensional field.

The foregoing sections dealt with the history and the systematics of the problem of equal temperament. Now the practical problem is before us to put the insight into practical use. Of course one wants to be in a position to have all the notes available

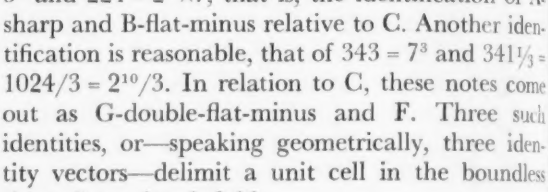


Fig. 1. Primary chords of four harmonics.

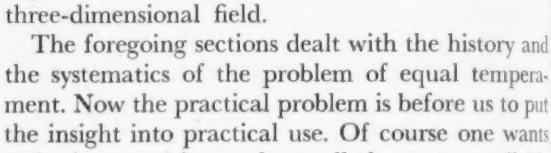


Fig. 2. Primary chords of four subharmonics.

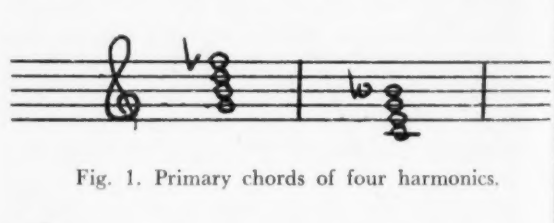


Fig. 3. Down a whole tone in five steps. Synonymous notations.

ready for use at one time. For an organ there is no specific difficulty in providing more pipes. The valves can be opened and shut by electropneumatic devices. The problem lies in the construction of the keyboard, and in the tuning of the pipes. The keyboard is shown in Fig. 5. There are two manuals, the second being placed behind the first, joining it at the same level, but rising at an increased inclination, steeper than the first. The keys are arranged like tiles on a roof, each key midway between a lower pair and a higher pair of keys. The keys in a horizontal row sound notes with distances of whole tones between them. Next to E one finds D and F♯. In stepping from E to F one finds the next higher horizontal row, which contains G, A, B, C♯, D♯, E♯. ... The key for E♯ is just in front of and lower than the key for F. This shows the situation of the steps of one diesis, that is, one-fifth of a tone. Keys for a series of notes such as D, D-plus, D-sharp, E-flat, E-minus, and E lie in a straight tier running away up the keyboard.

The keys have been colored to facilitate orientation. The lettered notes A, B, C, D, E, F, G have white keys, in conformity with ordinary keyboards. The black keys of the ordinary keyboard appear here in pairs, for example, C♯ and D♭, D♯ and E♭, and so forth. Other notes have blue keys. It happens that every white key lies in a straight rising tier between two blues: D between D-minus and D-plus, E between E-minus and E-plus (= F♭), F between F-minus (= E♯) and F-plus, and so forth. In all there are $7 \times \frac{1}{2}$ whites, 7×2 blues, and 5×2 blacks, or 31 keys per octave.

In order to facilitate playing, every note is represented by two keys. Therefore there are 11 horizontal rows. The keys in the lowest row have two replicas, one in the middle row and one in the highest row. Thereby the little finger and the thumb may find the keys they need in one range, the three long fingers may find their wants in a higher range. Pairs of keys in a similar relative position always bear the same interval. Therefore it is not necessary to change the fingering when one is playing various scales or melodies at different pitches. For transposing one simply displaces the hand in a uniform translation.

Besides the straight horizontal rows and the straight upward tiers there are skewed tiers. Rising to the right, one finds ascending major semitones: A-minus (=G×), A♯, B, C, D♭, E♭♭ (=D-plus), D× (=E-minus), E♯ (=F-minus), F♯, G, and so forth. Descending to the right, one finds in a straight line the minor semitones: C, C♯, C× (=D-minus), D-plus(=E♭♭), E♭, E♮, E♯ (=F-minus), F-plus-(=G♭♭), G♭, G♮. Thus the interval of the fifth C: G in one tier is divided into six steps; in the other tier it is divided into nine steps. The fifth being divided into six parts, it may also be divided into three equal parts. These are superseconds, which have the relationship 7 : 8. They are elementary steps in the music gamelang of Java and Bali. Again, the fifth may be divided into two equal parts. The result is a pair of "thirds" intermediate between minor and major thirds. These intermediate thirds make a very beautiful rendering of the cuckoo's call. In human civilization they are used in music of the Arabs according to the teaching of the famous medieval musician Zalzal. Thus the tricesimoprimal temperament provides means for a synthesis of many musics. The pedal (Fig. 6) shows an arrangement along lines similar to those of the manuals.

The tuning of the pipes is made by thirds. The deviation from perfect consonance is so small that, owing to the acoustical coupling by the intermediate air, two pipes—such as A and C♯—sounded together do not produce beats. Therefore an electric

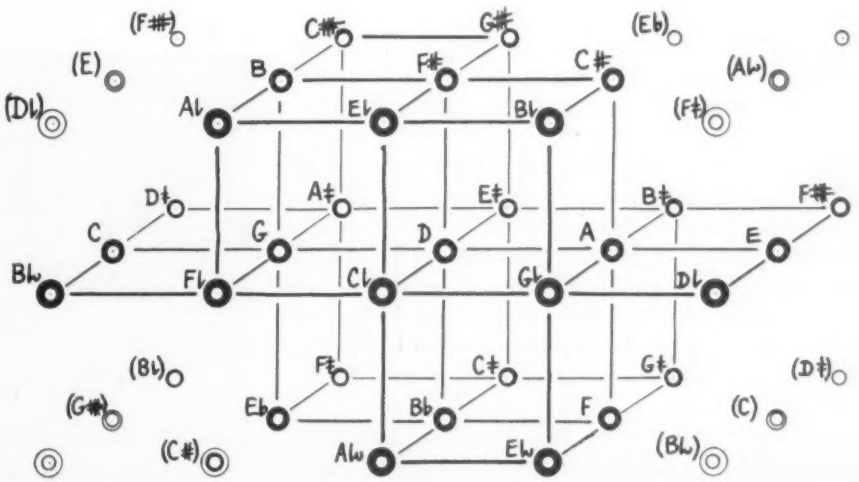


Fig. 4. Elementary set of 31 notes in the 3–5–7 lattice of harmonics.

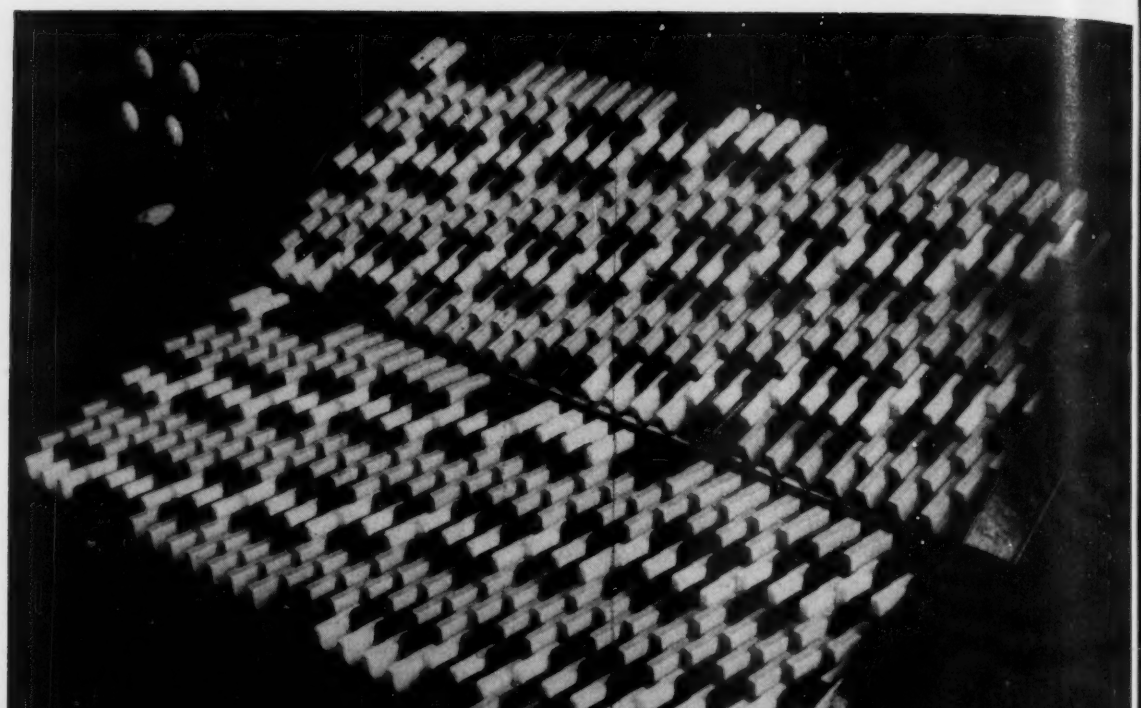


Fig. 5. The two manuals of the 31-keyed organ with white, black, and blue keys.

oscillating generator is first tuned against the pipe A to a perfect third. This is done by means of the Lissajous figure shown on the screen of an oscillograph which puts the sound of the pipe against the oscillations of the electric generator. If the generator is oscillating at the correct frequency, the pipe C♯ is sounded and its sound taken to the oscillograph. The pipe is adjusted until the resulting Lissajous figure shows the required number of beats, which might be any number, for example, one in seven seconds, or one in five seconds, as the case may be.

The 31-keyed organ has now been installed in Teylers Museum at Haarlem. It was built by B. Pels en Zoon, Alkmaar, Netherlands. It has a second console with ordinary keyboards. By electrical switchings, these keyboards can be connected to special selections of the complete scale. One of these selections is the one shown previously, with E♭ and G♯. It provides the notes as used and conceived by the 17th and 18th-century composers and it thereby restores the ancient beauty of their creations. This is the console looking back to the classical past. Recitals of ancient classical music are regularly presented to the public. In addition, the works of modern composers who avail themselves of the new possibilities (Jan van Dijk, Henk Badings, Paul Chr. van Westering, Martinus J. Lürsen, and Arie de Klein) have been executed at the console with the new keyboards. That is the console looking to the future.

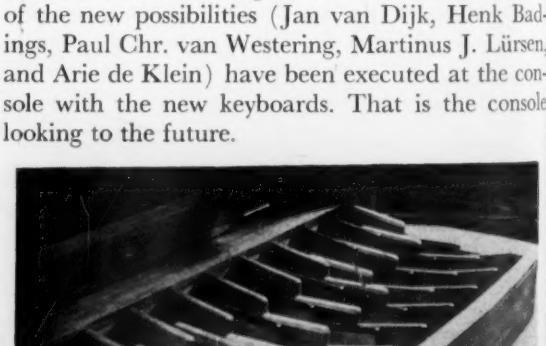


Fig. 6. Pedal of the 31-keyed organ.

Soviet Cosmology and Communist Ideology

MAXIM W. MIKULAK

Mr. Mikulak did his undergraduate work at City College of New York and received his master's degree at Columbia University. He was awarded the certificate of the Russian Institute of Columbia University for a research project on postwar Soviet attacks on "bourgeois" science. Mr. Mikulak resides at 629 East 5th Street, New York 9 and is continuing his study of the influence of Communist ideology on the development of Soviet science.

THE problem of studying the effect of Communist ideology on the Soviet sciences is a difficult one. The bulk of Soviet scientific research reveals no outward influence of the imposition of the official philosophy of the Soviet state. By and large, the work of Russian scientists conforms to Western methods and criteria of scientific investigation and criticism. Perhaps for these reasons, Western observers were startled to learn in the summer of 1948 that the Central Committee of the Communist Party suppressed the study of genetics primarily for *political* and *ideological* reasons. The suppression of genetics by Soviet authorities has been widely publicized. But only brief and scattered accounts have appeared in the non-Communist press of the ideological restrictions imposed on Soviet cosmologists—those astronomers concerned with the development of the universe at large.

When Russia was experiencing the Bolshevik Revolution in 1917, the scientific world was stirred by the revolutionary ideas inherent in Einstein's theory of relativity. It was at this time that Einstein expounded his notions on the "cylindrical" universe, the inseparability of space and time, the curvature of space and the finite nature of the spatial universe. Eventually Einstein's original concept of the universe was modified by de Sitter of Leiden, the Russian mathematician Friedmann, and the Belgian Abbé Lemaître. And further studies along this line were pursued, among others, by the British scholars Milne, Jeans, and Eddington.

Meanwhile, in the Soviet Union, particularly after Stalin consolidated his hold on the Communist Party, critical examinations of Western ideas were conducted. As early as 1932 Soviet astronomers were called upon to utilize Marxist-Leninist methodology in connection with their research, to exercise caution and criticism in dealing with the work of "bourgeois" scientists and not to bow blindly before "bourgeois" authorities of America and Europe (*1*). Apparently this warning went unheeded, for in 1937 Communist officials reviewed the situation on the Soviet astronomical front and discovered that it was riddled with "enemies of the people" who had not been liquidated (*2*). As a result, at least a half-dozen Russian astronomers vanished, including the prominent Boris Gerasimovich, who probably believed that astronomy could not be taught strictly on the tenets of Marxist-Leninist philosophy.

During this period the editorial board of the Communist Party journal, *Under the Banner of Marxism,* encouraged the publication of articles analyzing modern Western scientific theories in the light of Marxist-Leninist ideology. This journal had the complete support of the Central Committee of the Communist Party on matters pertaining to Soviet philosophy, society, and economics. The authority of the editorial board was rather extensive, for it could organize conferences and judge the correctness of scientific theories as it did in October 1939 when the board considered classical Western genetics as anti-Marxist.

Between 1937 and 1940, several ideological-astronomical articles appeared in *Under the Banner of Marxism* that revealed what Communist authorities and ideologists found objectionable in Western cosmological thinking. After reviewing the historical background of astronomical concepts, Soviet writers discerned certain "progressive" and "reactionary" trends. They noticed in the case of cosmology a long-standing struggle between those who espoused an infinite universe and those who espoused a finite universe. They then concluded that the traditional concept of the infinite universe is progressive and singled out the relativist theory of the finite expanding universe as a modern version of a reactionary, mystical, and idealist picture of the cosmos.

Relativist Universe

Prior to Einstein's cosmological consideration of the general theory of relativity, the universe was commonly regarded as infinite in dimension and mass and eternal in time. Olbers, who in 1826 reasoned that if space were uniformly filled with stars of average luminosity and shining forever then there should be enough radiation in space to illuminate the nocturnal sky, preferred to discard his paradoxical problem rather than reject the infinite universe. And in 1894, Seeliger introduced another paradox resulting from the infinite-type universe with a uniform distribution of matter. He applied Newton's law of gravity to this universe and found that the two were incompatible. Rather than accept a finite universe or an infinite universe without matter, Seeliger thought it wiser to modify Newton's law of gravity.

When Einstein arrived on the cosmological scene, he followed Seeliger's analysis and stated that the universe can be infinite only if the mean density of matter is zero or disappears. Such a universe appears contrary to experience. Unlike his predecessors, Einstein dismissed the infinite universe because "the hypothesis that the universe is infinite and Euclidean at infinity, is, from the relativist point of view, a complicated hypothesis . . . it is certainly unsatisfactory to postulate such a far-reaching limitation without any physical basis for it" (*3*).

Instead, after probing painstakingly, Einstein conceived a model of the universe in which space was curved, closed, and unbounded. This model, often referred to as the cylindrical universe, is static and contains an absolute, straight time-direction. De Sitter, however, constructed a spherical universe, devoid of matter, but managed to get rid of the troublesome straight or unique time-direction of the cylindrical universe. In 1922, A. Friedmann, the brilliant Russian mathematician, starting with Einstein's original relativist gravitational equations and assuming a uniform distribution of matter, arrived at a dynamic model of the finite unbounded universe with a varying radius. Five years later, Abbé Lemaître discovered that both the Einstein and de Sitter models are basically unstable. As soon as matter condenses in Einstein's universe, its radius begins to expand. And once matter is introduced into de Sitter's model, it also expands. Thus, the concept of the expanding finite universe was born.

The hypothesis of the closed but expanding universe avoids the paradoxes of Olbers and Seeliger. Furthermore, it helps to explain why the distant galaxies exhibit a red-shift in their spectral lines—the Doppler effect. The American astronomer, Edwin P. Hubble, conducted a series of investigations on the red-shift of the distant galaxies, an observation that is usually interpreted as evidence of recessional velocity. This means that our universe is in a rapid state of dispersion, which is in agreement with the Friedmann and Lemaître cosmological models.

Inasmuch as the universe is thought to be expanding, it seems that at one time it was in a high state of compression. From this starting point, Gamow, Alpher, and Herman, considering the relative abundance of atomic species, feel that at least 3 billion years ago the universe had a high density and that at this time the process of atom building occurred from subatomic particles. The Gamow-Alpher-Herman theory, based on the expanding universe, neatly explains the chemical composition of the observable universe. This is something that cosmologists who believe in the infinite steady-state universe have not been able to do with any satisfaction thus far.

Also, numerous methods and approaches have been developed indicating that our earth, moon, meteorites, clusters of stars, and galaxies have existed in more or less approximately the same state for not more than a few billion years (*4*). The expanding universe hypothesis can account for the upper age limit of most heavenly bodies. It seems that a cosmic catastrophe took place perhaps 5 or 6 billion years ago which set the stage for our present universe. Was it a cosmic explosion that gave birth to the expanding universe? From what little evidence we have, some cosmologists say *Yes*.

Even though the relativist idea of the closed universe is speculative and radical, it cannot be said that it is not useful. It provides a somewhat consistent hypothesis of the development of our cosmos, just as the Darwinian theory of evolution provides a cohesive picture of the development of a wide variety of living organisms. In spite of this, the Soviet ideologists, writing in *Under the Banner of Marxism,* stigmatized the Western concept of the expanding universe as reactionary.

Soviet Rejection of Relativism

What led the Soviet authorities to reject relativist cosmology? To answer this question adequately, it is necessary to understand the *modus operandi* of Soviet ideologists. Although Soviet attacks on certain aspects of Western astronomy had been made before 1937, these attacks were mainly of minor significance and received little play in Soviet publications. By 1937, the Communist Party ordered that all deviant persons, institutions, and ideas be exposed and purged. It then became the task of Communist thinkers to scrutinize modern scientific theories and to judge their progressive or

reactionary nature in the framework of Marxist-Leninist philosophy.

Although the apostles of Communism—Marx, Engels, Lenin, and Stalin—were not cosmologists, they evolved a *Weltanschauung* within which Soviet astronomers, if not all Soviet scientists, must operate. Surprisingly enough, Marxism-Leninism embraces such notions as the eternity of matter, the future development of matter, the interrelationship of the finite and the infinite, the nature of time and space, all of which are relevant to any study of the astronomical world.

Frederick Engels, Karl Marx's close friend and collaborator, devoted much energy and thought to these problems, and his conclusions are often treated by Soviet writers as official pronunciamentos on cosmology. His opinions on this subject reflected the advanced astronomical thinking of his time—the 19th century. To Engels, the universe had no creator and therefore had no beginning in time. He stated that the universe contained innumerable worlds in infinite space existing eternally and undergoing perpetual change. He regarded Clausius' second law of thermodynamics, when it was applied to the entire universe, as absurd, since this presupposed to him an act of creation. In place of the heat death of the universe—which is inevitable according to Clausius' law—Engels foresaw an unending succession of worlds whereby extinct stars fell into one another, produced tremendous heat from collisions, and then evolved into new stars (*5*). (This idea coincides fairly closely with the pulsating universe of Lemaître, who was attacked by Soviet ideologists as a bourgeois cleric!) And Lenin and Stalin subscribed to Engels' cosmology when both approvingly quoted Heraclitus, who believed that the world "was not created by any god or man, but was, is, and ever will be a living flame, systematically flaring up and systematically dying down" (*6*).

When the time was ripe for Soviet ideologists to criticize Western cosmology, they did so from a very definite Marxist-Leninist position. They strenuously objected to the relativist spatially-finite universe (*7*). They vehemently rejected all talk of the "age of the universe" and a finite time-scale as an example of blacksliding to the religious idea of creation (*8*). They pointed out that the relativist cosmologists do not conform to Engels' classic formulation on the relation of the finite to the infinite: the infinite is composed of nothing but the finite, and the finiteness of the material world leads to the contradiction of its infiniteness.

Having denounced the relativist approach to cosmology on philosophic grounds, the Soviet ideologists continued by also denouncing the methodological procedure of Einstein and his followers. Soviet writers do not really believe that Einstein was a bad fellow or that his ideas are basically unsound. Some went so far as to say that his theory of relativity was grounded in the dialectical materialism of Marx and Lenin, which is the highest praise Soviet writers can give (*9*). It is just that Einstein wandered off the straight and narrow Marxist-Leninist path of correct science.

First of all, Einstein and his successors were criticized for attempting to place the study of cosmology into a geometric strait jacket. The curvature of space on a large scale is viewed by Soviet authorities as a product of mathematical symbolism. One ideologist, B. Shafirkin, labeled this approach to cosmological space as tending toward subjectivism. He took pains to relate how Lenin, in 1908, warned that formalism is unavoidable when theoretical physics is transformed into mathematical physics (*10*). Subsequently, Einstein's application of non-Euclidean geometry to the universe is believed to be methodologically faulty by several Soviet thinkers (*11*). The same holds true for Minkowski's four-dimensional space, which has been called a mere mathematical apparatus (*12*).

The second major pitfall that Einstein and relativist cosmologists have fallen into, according to the ideologists, is the one of extrapolation. The latter charged that the theory of relativity is valid only for limited portions of the universe (*13*). The fundamental question involved here is, Can mathematico-physical laws, which appear valid on a local scale, be applied to the universe as a whole? This pressing problem continually haunts the astronomers who have limited data and must extend them to the vast expanse of the heavens, much of which probably is inaccessible to powerful telescopes (*14*). Nonetheless, Soviet authorities consistently maintain that the extension of the relativist physical laws to the universe as a whole is un-Marxist and therefore an unscientific procedure. They, however, offer no rigorous proof or scientific evidence that extrapolation of relativist laws has led to erroneous conclusions.

Because the spectral red-shift of distant nebulae, which is interpreted by many Western astronomers and physicists as receding velocity, forms an important cornerstone of the expanding-universe hypothesis, Soviet ideologists turned their attention to this phenomenon. They are reluctant to accept the customary Western interpretation of the red-shift (*15*). Although the Soviet writers do not deny outright that the red-shift of distant stellar systems may be due to the Doppler effect, they prefer to stress the interpretation that the spectral shift is probably the result of aging photons.

By working within the bounds of Marxist-Lenin-

ist philosophy, Soviet ideologists automatically restricted themselves. They severely attacked and abused the theories and ideas that contradicted the official state ideology of the Soviet Union. And once the Soviet ideologists reached certain conclusions on cosmological questions, these conclusions in essence became the Party line for Soviet astronomers.

Party Line

After the Communist purge of astronomical personnel, after a series of critical reviews of cosmological developments in the pages of *Under the Banner of Marxism*, after an exposé of the heretical practices of Soviet astronomers by the guardians of Party ideology, after all this, the astronomical sections connected with the Academy of Sciences of the U.S.S.R. decided to hold a 4-day meeting in December 1938 (*16*). The purpose of the meeting was to discuss the problems of modern cosmology. On the basis of several papers read before the session by Soviet astronomers, the participants passed some resolutions that are extraordinary in the annals of modern science (*17*):

> 1) Modern bourgeois cosmogony finds itself in a state of deep ideological crisis resulting from its rejection of the only true dialectical materialist concept of the infinity of the universe with regard to space and time.
>
> 2) The hostile work of the agents of fascism, which at one time managed to penetrate the leading positions in certain astronomical and other institutions and organs of the press, has led to outrageous propaganda of counter-revolutionary bourgeois ideology in literature.
>
> 3) The few existing Soviet materialistic works on cosmological problems, until recently, have remained in isolation and have been suppressed by the enemies of the people.
>
> 4) Scientific opinion at best has been molded in the spirit of indifference to the ideological aspects of present bourgeois cosmological theories. . . .
>
> 5) This session of Astronomical Groups considers that the exposure to the Soviet people of its enemies created the necessary conditions for the wide development of a new Soviet materialistic cosmogony. . . .

Attacks on Western astronomical theories were for the most part discontinued during World War II. In the immediate postwar period there existed a slight hope that differences between Western and Soviet astronomers could be resolved without resorting to acrimonious ideological denunciations and without dividing the scientific community into bourgeois and socialist camps. As a matter of fact, A. A. Mikhailov, corresponding member of the Soviet Academy of Sciences, stated at the International Astronomical Meeting in March 1946, "the more contact between scientists of various countries, the greater the influence it will have in furthering the development of world astronomy" (*18*).

This amicable atmosphere was quickly dispelled in November 1946, when the Central Committee of the Communist Party declared war on "all survivals of capitalism" in the consciousness of the Soviet people (*19*). The Politburo member, A. A. Zhdanov, was mainly responsible for instituting the back-to-Marxism-Leninism movement in the Soviet arts and sciences. Speaking *ex cathedra*, he was the first high-ranking Communist official to publicly deride "bourgeois" astronomy in the after-war years at the ideological-philosophical discussions of June 1947 (*20*).

Zhdanov's remarks about "bourgeois" astronomy would have probably passed unnoticed were it not for the bizarre biological sessions of the summer of 1948. These sessions were organized for the expressed purpose of uncovering the "materialist" and "idealist" features of Soviet biology and had a far-reaching effect upon the other branches of Soviet science. A leading editorial in the *Journal of the Academy of Sciences of the U.S.S.R.* called upon all Soviet scientists to come closer to the needs of socialist construction and to the Marxist-Leninist world outlook (*21*).

Soviet astronomers quickly responded to the postwar Party line in science. They organized their own conferences on 13 and 14 December 1948, at the Leningrad Section of the All-Union Astronomical-Geodetic Society, in order to examine present-day astronomy in terms of Communist ideology (22). By and large, these conferences were similar in several respects to those held in Moscow a decade earlier. Milne, Lemaître, Eddington, and Jeans were criticized for applying Einstein's theory of relativity to the universe as a whole. The Western concept of the expanding universe was viewed as a reversion to anthropomorphism. And a certain V. E. Lvov, who is something of a Party watchdog safeguarding the ideological purity of the Soviet physical sciences, said he wanted to underline the negative role of the propagators of the closed expanding universe, which is, as far as he is concerned, a "cancerous tumor that corrodes modern astronomical theory and is the main ideological enemy of materialist science."

As Soviet astronomers began toeing the Party line with vigor, the number of postwar ideological-astronomical articles increased in Soviet newspapers and in popular and semipopular scientific journals. Some of these articles reached a hysterical pitch. One Soviet astronomer even charged that the finite universe is actually the result of an "imperialist

ideology" that is trying to impose its own finiteness upon the heavens (*23*). And two Soviet professors of astronomy noted that the expanding universe hypothesis had received the blessings of Pope Pius XII. According to the authors, this was further proof that "bourgeois" cosmological ideas are fundamentally religious and therefore unscientific (*24*). The number of examples of such bitter ideological attacks on "bourgeois" astronomy could be multiplied many-fold. And up to the present time, there is no sign that the Party line for Soviet astronomers has undergone any basic change.

Effects on Astronomical Work

Some might argue that Soviet ideology has a superficial influence on the development of Russian astronomy. After all, intensive ideological attacks on Western cosmological theories have occurred during periods when Soviet leaders found it expedient to be anti-Western in orientation. What is more, ideological articles in Soviet scientific journals have been infrequently published; their scarcity is striking. And the number of professional Soviet astronomers writing ideological-astronomical articles is relatively small. Yet, it would be a serious error to conclude that Soviet astronomers merely go through the motions of paying homage to Marxism-Leninism and then continue to exercise unhampered freedom of inquiry into astronomical matters.

A systematic search of the major Russian-language astronomical and physical journals from 1930 to the present period discloses *almost a complete absence of any positive Soviet contribution to the relativist concept of the expanding universe.* In 1931, the Soviet journal *Achievements of the Physical Sciences* published a rare and neutral piece on relativist cosmology (*25*). Seven years later the author of this matter-of-fact article was condemned by a Soviet ideologist for being a member of the Leningrad-Moscow group of idealistic (or nonmaterialistic) physicists (*26*). The same ideological expert also lambasted other Soviet scientists for introducing to the Communist soil the theories of the expanding universe. The repercussions of these ideological criticisms were soon apparent when professional Soviet astronomers began to release "materialistic" works on astronomy in conformance with Marxist-Leninist philosophy.

In 1939, the Soviet scientist A. F. Bogorodsky published an interesting explanation of the red-shift (*27*). He showed how photons passing through a gravitational field can be expected to suffer a slight loss of energy, causing a spectral shift. The Communist astronomer M. S. Eigenson reviewed the problem of the surface brightness of the night sky and the infiniteness of the universe. He concluded that to reject the infiniteness of the universe is inadmissible on *philosophic* grounds (*28*). And in the postwar period Soviet astronomical textbooks and popular scientific books stressed the infinity of the cosmos in opposition to Western "religious and idealist" points of view (*29*).

Not only have Communist authorities reproached some Russian astronomers for subscribing to "bourgeois pseudo-science," but they also censured several editors of scientific journals for being neutral or not energetic enough in exposing the "corrupt" and "mystical" theories and practices of the West. For example, in 1938, the editors of *Achievements of the Physical Sciences* were taken to task for not carrying out the Marxist-Leninist line in all their scientific activities (*30*). More recently, the responsible editors of Russia's leading astronomical journal were criticized for failing to organize an adequate ideological struggle against "bourgeois" science (*31*).

Although there is ample evidence of the curtailment of the area of scientific inquiry owing to the imposition of Communist ideology on Soviet astronomers, it would be farfetched to suppose that Soviet astronomers are completely handicapped by Marxism-Leninism. If anything, the vast amount of Soviet astronomical research leans toward the uncovering and accumulating of empirical data. The stress is on practical work and away from speculative theories. Soviet astronomers operating within the ideologically safe areas of astronomy by and large exhibit the same approach and methodology that is used in Western astronomical research. The evidence for this is abundant and is obvious in every Soviet scientific journal. There is every indication that Soviet astronomers think very much the same as non-Soviet astronomers. But when Communist astronomers carry out the Party line there is a kind of double talk, a mixing of sound scientific work with political and ideological statements that is not supported by any true and scientific arguments. At these moments Soviet ideologists and astronomers invariably attribute to us a naivete that we actually do not possess or a medieval faith in outmoded religious and philosophic concepts that in reality have no place in our thinking.

It is unfortunate that Soviet cosmologists are not permitted to explore without fear all the mathematically possible models of the universe. One line of investigation might well lead to an *experimentum crucis,* which perhaps in turn will reveal the correct model of the cosmos. But it appears that the Communist authorities cannot afford to offer their astronomers the luxury of speculation. If Soviet

astronomers should become overly speculative and skeptical, it is quite possible that they might indirectly subvert the foundations of Marxism-Leninism and its all-embracing world outlook. Such a process is intolerable in a totalitarian state that professes monolithism. The very real and tragic implication resulting from the influence of Communist ideology on Soviet cosmology, at least to the Western mind, is the subservience of the Soviet astronomer to the Communist ideologist on questions related to the philosophic interpretation of observable data.

References and Notes

1. *Astron. Zhur.*, No. 1, 118 (1933).
2. *Izvestia*, 16, Dec. 1937, p. 4.
3. A. Einstein, *The Meaning of Relativity* (Princeton, 1953), p. 99.
4. See D. ter Haar, "The age of the universe," *Sci. Monthly* 77, 173 (1953).
5. F. Engels, *Dialectics of Nature* (New York, 1940), pp. 21–25 and 201 ff.
6. J. V. Stalin, *Problems of Leninism* (Moscow, 1940), p. 598.
7. *Pod Znamenem Marksizma*, No. 6, 102 (1937); No. 7, 130 (1938); No. 6, 118 (1939); No. 7, 128 (1940); No. 8, 75 (1940).
8. *Ibid.*, No. 6, 98, 103, 106 (1937); No. 7, 130, 143, 166 (1938); No. 6, 119 (1939); No. 7, 123 (1940).
9. *Ibid.*, No. 6, 102 (1937); No. 8, 62 (1940).
10. *Ibid.*, No. 7, 124 (1938).
11. *Ibid.*, No. 6, 119 (1939); No. 6, 126 (1940).
12. *Ibid.*, No. 7, 119 (1940).
13. *Ibid.*, No. 7, 124 (1938); No. 6, 126 (1940); No. 8, 67 (1940).
14. P. W. Bridgman, *The Nature of Physical Theory* (New York, 1936), pp. 109–110.
15. *Pod Znamenem Marksizma*, No. 6, 104 (1937); No. 7, 163 (1938); No. 7, 121, 127 (1940).
16. *Astron. Zhur.*, No. 2, 68–79 (1939).
17. *Ibid.*, pp. 77 ff.
18. *Vestnik Akad. Nauk S.S.S.R.*, No. 5–6, 103 (1946).
19. *Bol'shevik*, No. 21, 7 (1946).
20. *Voprosy Filosofii*, No. 1, 271 (1947).
21. *Vestnik Akad. Nauk S.S.S.R.*, No. 11, 7 (1948).
22. *Priroda*, No. 6, 71–77 (1949).
23. *Ibid.*, No. 7, 12–18 (1950).
24. *Izvestia*, 28, Jan. 1953, p. 3.
25. *Uspekhi Fiz. Nauk*, No. 1, 124–184 (1931).
26. *Pod Znamenem Marksizma*, No. 7, 146 (1938).
27. *Ibid.*, No. 7, 128 (1940).
28. *Astron. Zhur.*, No. 3, 49 (1939).
29. B. A. Vorontsov-Vel'iaminov, *Astronomiia* (Moscow, 1948), pp. 130 ff.; M. Ivanovskii, *Rozhdenie Mirov* (Leningrad, 1951), pp. 371 ff.; B. A. Vorontsov-Vel'iaminov, *Ocherki o Vselennoi* (Moscow, 1952), pp. 514 ff.
30. *Pod Znamenem Marksizma*, No. 7, 191 (1938).
31. *Astron. Zhur.* No. 6, 534–535 (1953).

Mushroom Coral

The mushroom coral (*Fungia*) is a good example of the analogy of form that may be found among widely divergent species—in this case, one a plant and the other an animal. The radiating lamellae of calcium carbonate that are characteristic of the mushroom coral are laid down in a manner that resembles the gills of some of the mushrooms. *Fungia* is one of the solitary corals, so-called because the entire growth is produced by a single large polyp. In contrast, the colonial corals are formed by many small polyps that are joined together by protoplasmic connections. The colonial corals, because of the multiple nature of their polyps, are able to form enormous structures, such as the great coral reefs. The solitary corals, on the other hand, are limited in their growth because of their single polyp—they seldom exceed 25 centimeters in diameter. The solitary corals are generally found lying loose—the polyps have many blunt tentacles, which they use in freeing themselves from the sand and silt that may settle around them. The photograph on this month's cover was made by A. M. Winchester from a dried specimen in his laboratory at Stetson University, Deland, Florida. He also supplied the explanatory note.

Changing Place of Soils in Agricultural Production

ROY W. SIMONSON

Dr. Simonson, who is director of soil classification and correlation for the Soil Conservation Service, U.S. Department of Agriculture, Beltsville, Maryland, is responsible for classification and nomenclature of soils in the United States. Following his academic work at North Dakota Agricultural College and the University of Wisconsin he was engaged in research and teaching at Iowa State College. Either as a representative of the U.S. Government or as an adviser to other governments, he has classified and mapped soils in the western Pacific islands, in central India, in the Caribbean, and in Brazil.

WHEN man ceased to gather his food and began to grow it, he was immediately forced to take some interest in the management and productivity of soils. Rudimentary at first, this interest has waxed and waned over the centuries. It has differed markedly from one society to another. Yet the primary interest of mankind in soils continues to rest on their capacity to support plants useful for food, fiber, and shelter. This is true in all societies, despite the varying importance of soils to agricultural production.

As part of the general growth of science, especially during the last century, there has been increased understanding of soils, their nature, properties, distribution, responsiveness to management, productivity, and use-suitability. This growth of knowledge, which is still far from complete, has accompanied a tremendous expansion in technology and industry. With these profound changes, the relative importance of soils in the production of food and fiber has changed. The present-day importance of soils in American farm production is a far cry from what it was 100 years ago or is in less fortunate countries now. The balance between soils and other factors in agricultural production has been shifted in industrial societies like those of the United States and western Europe. Moreover, the development of science and industry has changed the comparative advantages among various kinds of soils.

Soil Productivity

Primary production in agriculture is dependent on climate, soil, and the technology in a society. The yield of corn, or of any other plant, is governed by the nature of the soil supporting it, by the weather, and by the management practiced in its production. Insect pests and diseases also influence yields, although their effects are subject to modification through scientific management. The respective contributions of soils and management to yields vary with time and place, but both are essential to productivity. Some kind of management is required for agricultural production. The nature and intensity of that management reflect economic conditions and the level of technology in the society that uses the soil.

By *management* is meant the whole array of practices that go into agricultural production, whether they be few or many. In the agriculture of the United States, management includes the use of power machinery, improved crop varieties, fertilizers, insecticides, herbicides, and irrigation as well as many other findings of science and products of industry. Even within this one country, however, the range in kind and intensity of management is wide. Management may be simple, as it is in the production of livestock on ranches in Wyoming, or it may be complex, as it is in the citrus groves of Florida.

The range in management becomes even wider with the inclusion of societies outside the United States. In northern Ceylon, a tropical region, shifting cultivation is practiced by many of the inhabitants (*1*). Big trees are girdled, the underbrush is cut and burned, the soil is stirred with a simple hoe, and the crops are planted and harvested by hand. None of the machinery, fertilizers, and insecticides taken for granted in the United States are available to the primitive cultivator in northern Ceylon. He must depend on the natural fertility of the soil and on the plant nutrients released by the burning and decay of vegetation. After 1 or 2 years of cultivation, his ground must be abandoned for a new clearing. With his limited resources, the management alternatives are few.

For him, productivity is almost entirely dependent upon the nature of the soil. Soils low in plant nutrients or in water-holding capacity and soils of poor tilth cannot be productive for cultivators who must rely on shifting cultivation and the level of technology it represents.

By way of contrast, the productivity of a soil when it is first plowed may bear little relationship to its potentialities in a modern, industrial society. With the technologic resources of such societies, some of the most productive soils, in an economic sense, are poor when they are first plowed. For example, citrus fruits are produced on the Lakeland and Blanton sands in Florida under complex and costly management. Most of the nutrient elements necessary for plant growth are added in one way or another, water is supplied through irrigation, twigs from the best fruit varieties are grafted to hardy root stocks, and sprays are used for the control of insects and diseases. When first cleared, the Lakeland and Blanton soils are not expected to produce crops worth harvesting unless fertilizers are used. With the management possible through the findings of science and the products of industry, however, these sands in Florida can be made highly productive in a short time. Such soils are said to be highly responsive to management. Their productivity can be high in a modern, industrial society, whereas it would always be low in primitive societies. This further illustrates the importance to soil productivity of the whole complex of technology in the society that uses it for agricultural production.

Natural Differences among Soils

The marked disparities in levels of technology among societies can be matched by differences among soils. These differences are both local and regional. Some are large, and some are small. They reflect differences in the history of soils, most often their natural history. For example, the amount of rain that falls each year, the hours of sunshine, the kinds of plants that grow, and the slope of the land surface affect the nature of the soil at any given spot. Each soil is a function of the five major factors of climate, living organisms, topography, parent rocks, and time. In regions such as the interior of Brazil, natural history has determined the present character of the soils. In the Orient, where soils have been used intensively for centuries, the history under cultivation has been more important.

Whatever the causes, each soil has a combination of characteristics peculiar to itself, which distinguishes it from other soils. At the same time, every soil has some properties in common with many others. The existence of differences and similarities, expressed to various degrees, provides the basis for classification of soils. Present classification systems, such as the system used in the United States (*2*), include narrow classes to distinguish the kinds of soils within small areas and broad classes that can be used for the study of the soils of a continent.

Every farm consists of several local kinds of soil or soil types important to management, productivity, or both. Farms in the United States include anywhere from three to six soil types within their boundaries, on the average. For the country as a whole, there are several thousand soil types. Differences among the soil types on a single farm may be large, as they are between Congaree fine sandy loam in the small floodplain and Ashe stony loam on the adjacent mountainside in the Smoky Mountains of western North Carolina. They may be small, as they are between Tama silt loam and Downs silt loam on the same ridge crest in central Iowa. By and large, local differences among soils are smaller than regional differences. Many of the soils within any small area, such as a county, usually have some important features in common.

Soil types occur in characteristic groups and patterns in certain geographic areas. The several thousand soil types in the United States are not scattered indiscriminately from one end of the country to another. For example, Decatur silt loam occurs in eastern Tennessee, northwest Georgia, northern Alabama, southwest Virginia, and south-central Kentucky. Barnes loam is an important soil type in eastern North Dakota and also occurs in adjacent parts of Minnesota and South Dakota. Neither of these two soil types is the only one in the region of its occurrence, but it is restricted to that region. Furthermore, the soil types of eastern Tennessee do not occur in North Dakota, Oregon, or Maine, or vice versa. The occurrence of characteristic groups and patterns of soil types in specific geographic areas is thus reflected in regional differences among soils, differences that have great importance to agricultural production.

In order to deal with soils of a large land mass, the numerous soil types are considered in broad classes, each consisting of many soil types. Approximately 40 of these broad classes, known as great soil groups (*2*), have been recognized in the United States through studies extending over some 50 years. The general pattern of distribution of these great soil groups in the United States is shown in Fig. 1 (*3*). Considered collectively, these 40 great soil groups have wide ranges in many of their characteristics or properties. They also have wide ranges in qualities such as fertility, tilth,

available moisture capacity, and susceptibility to erosion. For example, the differences between the Chernozems of the spring wheat region and the Red-Yellow Podzolic soils of the Cotton Belt are striking.

The well-drained, slightly weathered, black soils known as Chernozems are among the naturally most fertile in the world (Figs. 2 and 3). On the other hand, the well-drained, red and yellow, leached soils of the Red-Yellow Podzolic group (Figs. 4 and 5), are inherently poor but do have large potentialities under modern scientific management. This range in regional differences can be widened further by including the Podzols and Desert soils. The Podzols are light-colored, leached and acid soils formed under forest in northern Michigan and northern New England. Less strongly weathered than Red-Yellow Podzolic soils, the Podzols lack the potentialities of the latter. They lie in a region with far less favorable climate. Desert soils are light-colored, virtually unleached, and alkaline in reaction. Formed under shrub vegetation in Nevada, Arizona, and nearby states, the Desert soils are commonly high in nutrient elements other than nitrogen but require irrigation for high productivity.

Additional examples could be given, but these indicate kinds and magnitude of differences among soils in the United States (*4*). Similar differences also prevail in other parts of the world, as is suggested by a schematic soil map in Fig. 6 (*5*). These differences and the climates with which they are associated are of great importance to the usefulness of soils and to their possible productivity.

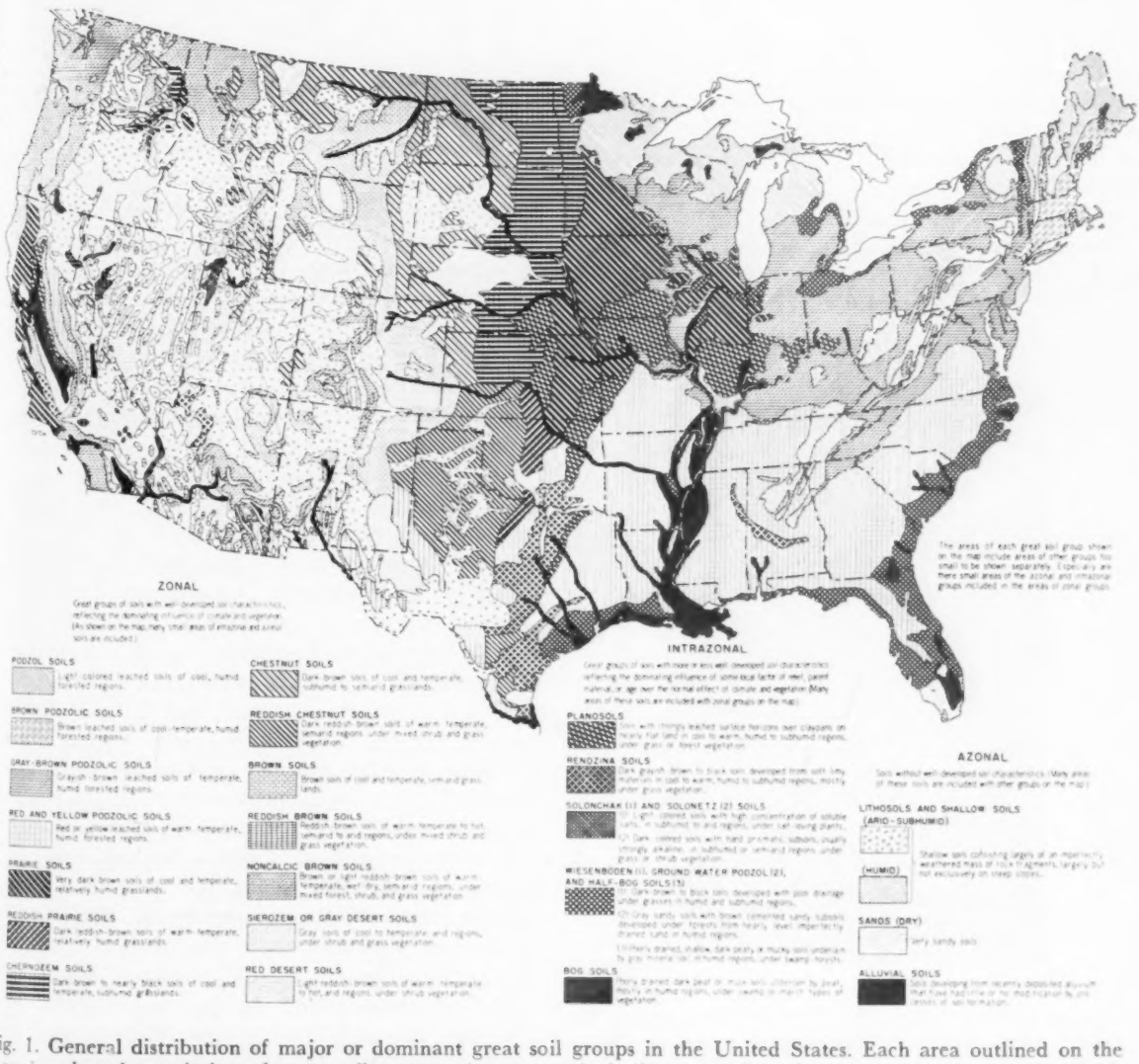


Fig. 1. General distribution of major or dominant great soil groups in the United States. Each area outlined on the map is a broad association of great soil groups, the pattern including a number of minor soils that are not named.

Changes in Soils with Use

The use of soils in agricultural production commonly means a change in the environment from that in which the soils were formed. Man clears the forest and plows the prairie. He drains extra water out of some soils and adds extra water to others through irrigation. In extreme cases, he may rearrange soils entirely, as has been done in rice paddies of Japan and with peats and dune sands in Holland. More changes are usually made in chemical than in physical properties. Furthermore, the changes tend to reduce differences among soils as they are used for agricultural purposes (*6*).

Little is as yet known about changes in soils of the United States because of their utilization for agriculture. Striking changes have occurred in a few soils, and it seems probable that small changes of one kind or another have occurred in many soils.

The most dramatic change in soils under agricultural use in the United States has been erosion of some that were improperly used or poorly handled. Such changes are spectacular, as for example the development of intricate networks of gullies on small areas. Very badly eroded soils are often lost to cultivation. More commonly, soil erosion may increase costs of production for the future.

Fertility depletion and deterioration of structure seem to have occurred in Chernozems and similar soils in North America. Organic matter contents in the surface layers of Chernozems have been lowered about one-third from original levels during a period of cultivation ranging from 40 to 80 years (*7*). The naturally strong granular structure in these soils also seems to have deteriorated, although the evidence on this score is less clear.

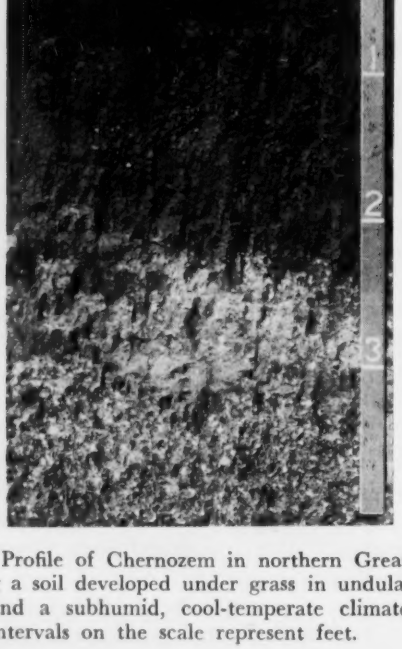


Fig. 2. Profile of Chernozem in northern Great Plains, showing a soil developed under grass in undulating uplands and a subhumid, cool-temperate climate. Numbered intervals on the scale represent feet.

Generally speaking, fertility depletion was a sensible course of action for the settlers on Chernozems and similar soils. A part of the initial large store of fertility is far more useful to society after it has been converted into homes, schools, roads, and the like, than it would have been locked up in the soil. Furthermore, it was impractical to maintain the native levels of fertility in Chernozems and similar soils, high as they were. A level of fertility somewhat below the natural one becomes the optimum in the new environment, which includes cultivation, even as a level of fertility appreciably above the original is necessary for the continued use of the Red-Yellow Podzolic soils of the southeastern United States (*6*).

In contrast to the Chernozems that have not been improved under use, the soils in western Europe have gained in productivity during the last two centuries (*8*). Soils of western Europe are closely related to the Gray-Brown Podzolic group, represented by soils formed under oak-hickory forest in the northeastern fourth of this country. These soils are of moderate to low fertility under natural conditions. On such soils in western Europe, grain yields remained stationary between 6 and 10 bushels per acre during the Middle Ages. There were no important changes, either up or down, from the fall of Rome until the French Revolution. During the last half of the 18th century, crop rotations such as the Norfolk system in Britain were developed. The practice of adding manure and chalk to the fields also spread. Yields of grain in western Europe began to rise and by 1850 reached 14 bushels per acre in France, 16 bushels per acre in Germany, and 20 bushels per acre in Britain. They have risen further since that time, and the average yield in Britain is now 35 bushels per acre. Approximately half of the increase in grain yields came before chemical fertilizers were introduced about a century ago (*8*). During the last 200 years, the soils of western Europe have been improved substantially, in spite of the fact that they have produced well during this period. The growth of knowledge about soils and their management, as well as the growth of industry, has permitted society to produce good crops and to make the soils more productive at the

Fig. 3. Landscape from Chernozem, or spring wheat, region of the northern Great Plains, showing common small grain crops.

same time. Comparable examples can be drawn from other parts of the world.

In order to assess the full effects of using soils on their probable character in the future, they will need to be studied where they have been cultivated for centuries. Even where soils have been used for many centuries, there are few, if any, systematic studies of the changes that have been wrought. Some fragmentary observations are available. In the rice paddies of China (*9*) and Japan, the soils now have characteristics common to wet soils of humid regions. In many instances, they have been greatly altered from what they originally were, as is clear from differences between soils inside and soils outside the paddies. Certain soils in western Europe have been profoundly changed by the additions of organic refuse during the centuries since Roman times.

Soil may even show the effects of past use long after that use has been discontinued. A few years ago, I saw soils in northern Ceylon that were mottled and bore other marks of poor drainage, although they now seem to be well drained and support native forest. Historical records show that the area was in rice paddies for several centuries in an earlier culture. Paddy rice has not been grown for the last four or five centuries, but the soils still bear visible marks of the earlier period of rice culture. In all probability, they bear invisible marks as well. The Regur soils of the Deccan plateau of India have been cultivated for centuries, most of them without irrigation. These soils show few marks of that use, except for some that are eroded. Yields have been low, however, in an agriculture that relied on oxen for farm power and had few fertilizing materials available (*10*).

These few illustrations suggest that soils may be changed much or little as man uses them. The changes may be slight, as they appear to have been in the Regur soils of India (*10*), or they may be profound, as they have been in the paddies of China (*9*). Changes may be either for better or for worse. This much is already known. In a sense, soil is thus a renewable resource. It can be used for the production of food and fiber and allowed to deteriorate. Or it can be used and improved at the same time. Soils have gone in both directions in the present Western culture, as they probably also have in earlier cultures. More knowledge is required about the directions and rates of change in soils under different kinds of use and management, however, before the best methods for achieving sustained production of useful plants in the future can be selected.

Use-Suitability of Soils

Each type of soil has a certain range in its use-suitability, depending in part on the level of technology in a society. This range can be established approximately but not precisely with the present

understanding of soils. For a given level of technology, the range is narrow for some soils and wide for others. Many of the Desert soils in Arizona are useful for short-season grazing and for little else in our society. The possible kinds of uses are extremely limited. Having more leeway, the Chernozems of southwestern Minnesota can be used for corn, small grains, hay, or pasture. Returning to an earlier illustration, a mountainside of Ashe stony loam in the Smoky Mountains of North Carolina is best suited for forest and may produce some pasture. An adjacent floodplain of Congaree fine sandy loam can be used for a wide variety of vegetable crops, small fruits, cereals, hay, pasture, or forest. Thus, the possible range of uses is wide for some soils and sharply restricted for others, even within a society rich in technologic resources.

Like differences among soils, the distinctions in their use-suitabilities are regional as well as local. The range in use-suitability of the Chernozems in the spring wheat region of the United States is narrow as compared with that of the Red-Yellow Podzolic soils of the Cotton Belt. Although the latter soils are much less fertile naturally, they will produce a far greater variety of crops under modern scientific management. Red-Yellow Podzolic soils can also be used for several kinds of pasture and for forest. In the Chernozem region, the shortness of the growing season and the amount of solar energy falling on an acre during the crop season are factors that limit potential production. The costs of changing these factors are tremendous as compared with the cost of improving fertility levels in the Red-Yellow Podzolic soils. The range in use-suitability of individual soil types is greater as a rule in the southern half of this country than it is in the northern half. It is also commonly greater in humid regions than in arid and semi-arid regions.

Fig. 4. Profile of a Red-Yellow Podzolic soil in southeastern United States, a soil formed under forest in rolling uplands and a humid, warm-temperature climate. Numbered intervals on the scale represent feet.

Generally speaking, use-suitability is affected as much by the level of technology in a society as it is by the character of soils. The availability to farmers of the findings of science and the products of heavy industry has an important bearing on how a soil type may be used successfully. By *availability* is meant not only the physical possibility of obtaining information and equipment but also the costs of these things in terms of money or labor. It should be recognized that useful plants can be grown on any soil if costs are disregarded. To take an extreme example, a greenhouse could be built over any small spot in the world. It could be heated and lighted if it were near the poles, and it could be cooled artificially if it were in the tropics. Plants grown in the greenhouse could be sprayed as needed with balanced nutrient solutions. Obviously, such practices are not economical and will not be adopted in agricultural production. Within the limitations set by economic conditions in a society, however, the general level of technology is important to use-suitabilities of soils, because it does fix the availability of scientific research and industrial products in primary agricultural production.

Red-Yellow Podzolic soils occur in the southeastern United States and in the Shan states of Burma. Continuing crop production is practiced on the soils of the southeastern United States, as contrasted with shifting cultivation of similar soils in Burma. Moreover, there are tremendous differences in yields obtained. For the last few years, many farmers in North Carolina and Virginia have produced yields of 100 bushels of corn or more per acre on Red-Yellow Podzolic soils. These farmers have used much lime and fertilizer, and they have also had expensive equipment. The cultivator who clears a patch of similar soil in Burma and plants sweetpotatoes or casava must rely on its inherent fertility plus nutrients released by destruction of existing vegetation. His garden patch is no more than one-twentieth of the size of a small field in Virginia, and his yield is equivalent to one-tenth or less of the 100 bushels of corn per acre. Without better education plus the prod-

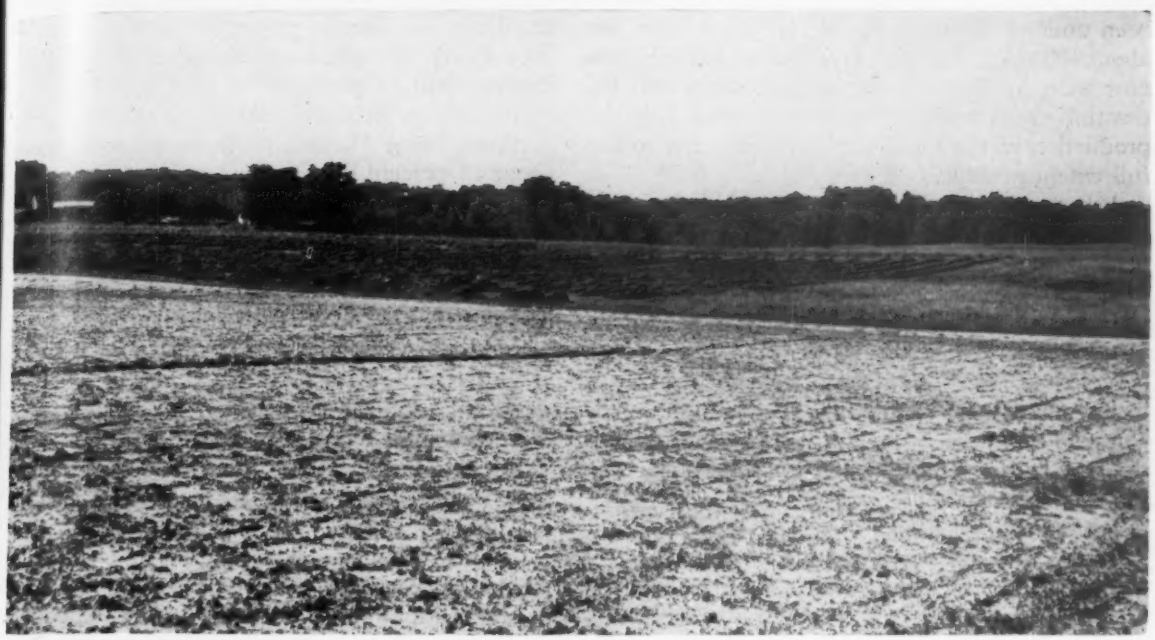


Fig. 5. Rolling landscape from Red-Yellow Podzolic region, showing light-colored surface of the plowed field in foreground with hay and forest beyond.

ucts of industry and the findings of science, the cultivator in Burma has no alternative to his present practice of shifting cultivation. The leached, infertile soils on which he lives are not suitable for continued cultivation unless he can obtain the scientific knowledge and the industrial products that are available in more fortunate societies.

Another illustration of the importance of agricultural technology can be drawn from differences in the use of some red soils formed from limestone in east Tennessee and on Okinawa. The soils in the two widely separated places are very closely related, so far as can be determined by field studies and laboratory analyses. Furthermore, some of the closely related soils in both places are marked by numerous outcrops of limestone, generally a few feet tall. These outcrops are only slight hindrance to tillage with a mattock, the common practice on Okinawa. In east Tennessee, on the other hand, the outcrops preclude the use of power machinery or draft animals, which makes the soil unsatisfactory for cultivation. In fact, the soil is used only to a limited extent, for pasture. The range in use-suitability within the technologic framework of present societies is wider for the soil on Okinawa than it is for the similar soil in east Tennessee. Partly, also, there is a differing need for food production in the two places. The contrast in utilization emphasizes further the importance of the whole complex of technology in a society to the possible use of its soil resources. In short, the use-suitability, management requirements, and yields obtainable depend in part on the soil and in part on the technologic and scientific resources available to people.

Sustained Production and Technology

The history of mankind during the last 5000 years, especially in the Orient, clearly shows that soil will produce for a long, long time. China had a land classification based on soil productivity 50 centuries ago (*11*), which implies not only that the soils were producing useful plants at the time but that differences in productive capacities were being recognized. India has also maintained a large population for a long time. Populations in the two countries were not well fed, and they were subject to famines. Despite this, however, the soils have been producing substantial quantities of food and fiber for many centuries. Experience in western Europe within historic times is parallel to that of the Orient in many ways. Production of food and fiber from the soil is clearly possible for long intervals of time without benefit of scientific research or industrial products, but the levels and efficiency of production are below those required today (*8*). World population is larger now, and it is generally known that higher levels of production are possible (*12*).

Examples of more recent experience also suggest that long-time production from soils is clearly

possible. Some of the soils in Puerto Rico have been under cultivation, mainly for sugar cane, for about 400 years. The yields on those soils are higher now than in the past. Research findings and industrial products have been introduced into the production of the sugar cane, although not to the full extent possible.

A better known illustration of stability of production is the continuous growing of wheat on a field at the Rothamsted Experimental Station in Great Britain for the last 100 years. The average yield from the check plot, which has received no fertilizers, is now 12 bushels per acre (*13*), which is very near the world average. A steady state seems to have been reached in wheat production on the check plot, where the soil is comparable to well-drained types of the Gray-Brown Podzolic group in Ohio and Indiana.

Farm production has increased sharply in the United States during the last 15 years (*14*). This is a period in which the accumulated results of research have found their way rapidly into farm practice. In 1948, farm production in this country was 40 percent more than it was, on the average, for the 5-year period of 1935–39. The present level of production is already above that of 1949, and the increase may reach 60 percent of 1935–39 in 1955. This has been accomplished without adding new acres of soils. Yet the increase is by no means a realization of the full potential within present science and technology (*15*). If all farmers in the United States were operating as efficiently as the uppermost 10 percent are at the present time, the increase in production over 1935–39 would be at least 100 percent. It would probably be more. This general improvement in efficiency would be hard to achieve, but the comparison does spotlight the gap between what is and what could be.

The recent increases in farm production have not been uniform among agricultural commodities (*14*). For example, the average yield of potatoes per acre has increased 95 percent—from 117.5 to 228.9 bushels from the 5-year period of 1935–39 to that of 1947–51. During this same interval, the average yield of corn per acre increased 46 percent, cotton 24 percent, and wheat 26 percent. The increase in the average yield of wheat for the country as a whole has been from 13.2 to 16.6 bushels per acre, not a large absolute increase but an appreciable relative one.

The recent increases in agricultural production have not been uniform over the country (*14*). The largest increase for any section, as compared with production in 1935–39, has been 25 percent in the South, the region in which Red-Yellow Podzolic soils are extensive and important. Recent research and farm testing in this section have demonstrated that large increases in production can be obtained from heavy nitrogen fertilization of corn and the improvement of pastures for year-round grazing. In the region dominated by the Chernozems (the northern Great Plains), production has increased about 14 percent during the last 15 years. For the country as a whole, more than half of the increase has come from regions of naturally infertile soils and perhaps one-tenth of it from regions of naturally fertile soils. These increases have been accomplished with small changes in patterns of agricultural production. They do not begin to reach potentials within present resources of scientific skills and industrial production.

The marked increase in total farm production in this country during the last two decades and the regional distribution of the increase emphasizes the growing importance of technologic resources. By *technologic resources* is meant the application in farm practice of the findings of science and the products of industry. Examples of important scientific findings are hybird corn, herbicides such as 2,4-D, insecticides such as DDT, and antibiotics for livestock. Industrial products of importance are fertilizers, farm machinery, insecticides of various kinds, and the several herbicides now available.

The wide use of 2,4-D for weed control in recent years is a single striking example of the combined importance of science and industry to farming today. In 1945, less than 1 million pounds of 2,4-D was produced in the United States, whereas 6 years later production was slightly more than 17 million pounds (*16*). Without scientific research, the usefulness of herbicides for weed control would still be unknown. Without modern industry and transportation, herbicides would not be available in quantities and at prices to permit their wide use on farms.

A similar indication of the importance of technologic resources lies in the increased use of fertilizers. From 1931 to 1951, consumption of fertilizers in the United States increased about 2½ times, going from 8,425,000 to 20,900,000 tons (*16*).

A striking illustration of the possible effects of modern technology in primary agricultural production can be drawn from rice-yield contests in northern Hyderabad State, India. The average yield in the northern part of that state ranges between 800 and 1200 pounds of paddy rice per acre. This is the level of production possible with a minimum of the products of modern industry such as fertilizers and machinery. In the fall of 1951, the winners in the yield competition sponsored by the agricultural advisory service got 6000

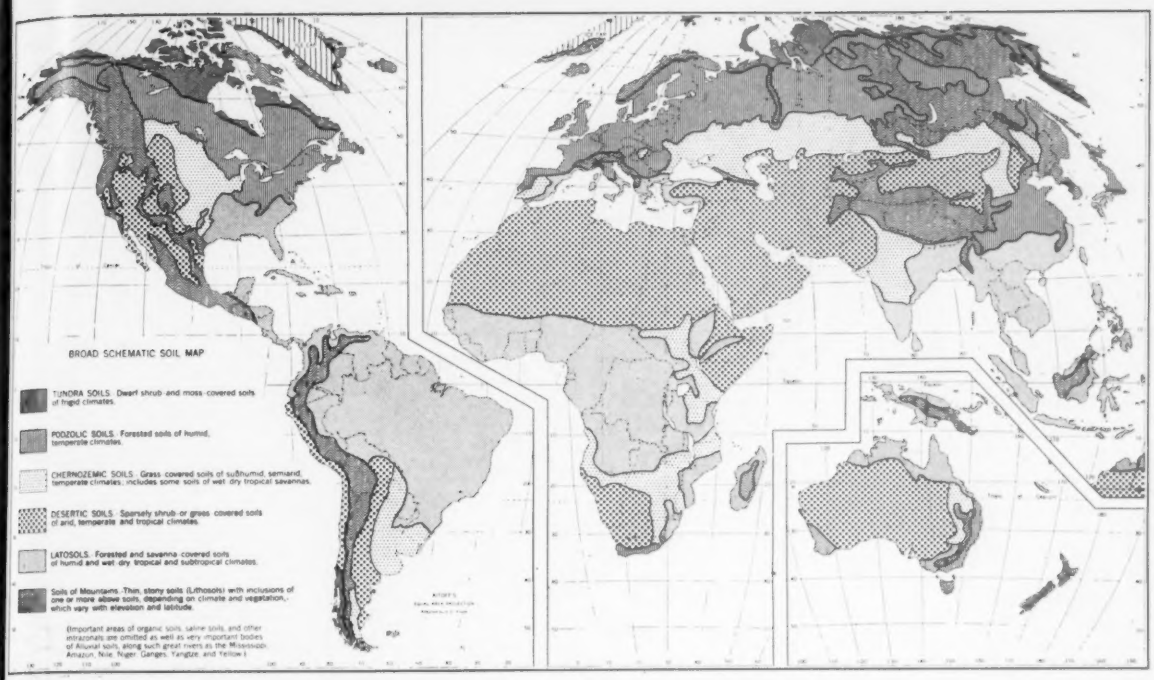

BROAD SCHEMATIC SOIL MAP
TUNDRA SOILS. Dwarf shrub and moss-covered soils of frigid climates.
PODZOLIC SOILS. Forested soils of humid, temperate climates.
CHERNOZEMIC SOILS. Grass-covered soils of subhumid, semiarid, temperate climates; includes some soils of wet dry tropical savannas.
DESERTIC SOILS. Sparsely shrub- or grass-covered soils of arid, temperate and tropical climates.
LATOSOLS. Forested and savanna-covered soils of humid and wet dry tropical and subtropical climates.
Soils of Mountains. Thin, stony soils (Lithosols) with inclusions of one or more above soils, depending on climate and vegetation, which vary with elevation and latitude.
(Important areas of organic soils, saline soils, and other intrazonals are omitted as well as very important bodies of Alluvial soils, along such great rivers as the Mississippi, Amazon, Nile, Niger, Ganges, Yangtze, and Yellow.)

Fig. 6. Broad soil zones of the world. Each zone has generally uniform processes of genesis reflected in the well-drained soils with undulating to rolling topography. Within each zone are many soils differing much or little from the well-drained soils.

to 7000 pounds of paddy rice per acre. These cultivators used heavy fertilizer applications and improved rice varieties. Methods of tillage, planting, and harvesting remained unchanged. Yields increased tremendously with little change in the agricultural arts, although substantial use was made of scientific and industrial resources within established patterns of operation. It does not follow from this one illustration that similar increases would be obtainable on all soils producing rice in Hyderabad. It does seem, however, that sharp increases in production would be possible in many paddies now yielding at low levels.

On the whole, the soil has less control over productivity now than it did prior to the marked growth of scientific knowledge and of technology. In societies with access to the findings of modern science and the products of heavy industry, the whole complex of management is relatively more important and the soil is relatively less important to primary agricultural production. Conversely, the nature of the soil was and is more important to the production of food and fiber in a primitive society than it is in a modern, industrial society. Many more alternatives are open to the cultivator in an industrial society. He has access to many more "tools" with which to achieve satisfactory production. He is far less dependent on inherent fertility and other properties of the soils as he finds them, because he has much more opportunity to modify the soils more nearly to fit his needs.

By and large, the soil has more control over productivity if the agriculture is extensive rather than intensive. Where the inputs of labor and materials per acre are low, the soil will commonly have more importance to production than it will where the level of inputs is high. Soil differences will, of course, continue to be important where the levels of input are high and the margins of profit are small. Thus, the nature of the soil and the general level of technology remain important in all instances, but the comparative roles of the soil and of management change from one situation to another.

Opportunities for efficient sustained production from soils will depend far more on technologic resources in the future than they have in the past. In countries such as the United States, research results are moving from the laboratories and experimental fields to operating farms at an accelerating rate. Even in this country, however, there is a substantial lag between the completion of research and its appearance in general farm practice. This lag is even greater where farm people have enjoyed little education and are near the margins of subsistence.

Regardless of present education and level of subsistence, people accept new practices or modifi-

cation of old ones more readily when these can be fitted into present modes of operation. This was illustrated by the rapid acceptance of hybrid corn in the Midwest some 15 years ago. Marked departures from established practices or changes in agricultural methods that require capital investment are accepted far less readily. Furthermore, changes that do require capital investment are possible only for people who are above the margin of subsistence and do have available resources.

Improvements in many types of agriculture are possible without drastic changes in the present agricultural arts. Thus, the improvement of primary agricultural production and the achievement of sustained production in any society require consideration of the technologic resources as well as the soil resources. Neither factor can be safely neglected.

References and Notes

1. E. J. Livera, "Productive land use in Ceylon" *Trop. Agr. (Trinidad)* **31,** 188 (1954).
2. This system of classification is described and discussed in *Soils and Men,* 1938 Yearbook of Agriculture (U.S. Dept. Agr.) and modified in a symposium that appeared in *Soil Sci.* **67,** No. 2 (1949).
3. C. E. Kellogg, *Climate and Man,* 1941 Yearbook of Agriculture (U.S. Dept. Agr.).
4. E. Winters, and R. W. Simonson, "The subsoil," *Advances in Agron.* **3,** 1 (1951).
5. A. C. Orvedal, J. Kubota, H. M. Smith, "Major soil profiles and their relationships with climate," in *Frost Action in Soils—a Symposium.* Highway Research Board Spec. Rept. 2 (1952).
6. R. W. Simonson, "The soil under natural and cultural environments," *J. Soil and Water Conservation* **6,** 63 (1951).
7. L. F. Puhr and O. Olson "A preliminary study of the effect of cultivation on certain chemical and physical properties of some South Dakota soils," *S. Dakota Agr. Expt. Sta. Bull.* 314 (1937).
8. C. E. Kellogg, "Opportunities for world abundance," *J. Politics* **13,** 325 (1951).
9. J. Thorp, "Soil Changes resulting from long use of land in China," *Soil Sci. Soc. Amer. Proc.* **4,** 393 (1939).
10. R. W. Simonson, "The Regur soils of India and their utilization," *Soil Sci. Soc. Amer. Proc.* **18,** 200 (1954).
11. M. Ping-Hua Lee, "The economic history of China with special reference to agriculture, *Columbia Univ. Stud. History, Econ. and Public Law* **99,** No. 1 (whole No. 225), 33 (1921).
12. M. K. Bennett, *The World's Food* (Harper, New York, 1954).
13. E. J. Russell, *Soil Conditions and Plant Growth* (Longmans, Green, New York, ed. 8, 1950) p. 59.
14. B. T. Shaw, "Role of research in meeting future agricultural requirements," *Agron. J.* **45,** 85 (1953).
15. S. E. Johnson, Chm., "Agriculture's capacity to produce," U.S. *Dept. Agr. Inform. Bull.* **88** (1952).
16. R. K. Smith, Chm., Agricultural Statistics (U.S. Dept. Agr., 1952), pp. 705, 709.

The reading of the works of two men, neither of them imbued with the spirit of modern science—neither of them, indeed, friendly to that spirit—has placed me here today. These men are the English Carlyle and the American Emerson. I must ever gratefully remember that through three long cold German winters Carlyle placed me in my tub, even when ice was on its surface, at five o'clock every morning—not slavishly, but cheerfully, meeting each day's studies with a resolute will, determined whether victor or vanquished not to shrink from the difficulty. I never should have gone through Analytical Geometry and the Calculus had it not been for those men. I never should have become a physical investigator, and hence without them I should not have been here today. They told me what I ought to do in a way that caused me to do it, and all my consequent intellectual action is to be traced to this purely moral source.—JOHN TYNDALL, *Fragments of Science,* Vol. II, p. 96.

Are There Rules for Writing History of Chemistry?

AARON J. IHDE

The author is associate professor of chemistry and integrated liberal studies at the University of Wisconsin, where he received his doctorate in 1941. His interest in the relationship between science and cultural developments has resulted in the articles "The inevitability of scientific discovery," and "Responsibility of the scientist to society" which appeared in The Scientific Monthly *in December 1948 and November 1953, respectively. The present article was presented by invitation at the* Symposium on the Writing of the History of Chemistry *that was sponsored by the Division of the History of Chemistry, American Chemical Society, at the 126th national meeting of the society in New York, September 1954.*

THE history of science until recently received little attention from traditional historians whose energies were devoted so largely to the conflicts of popes, emperors, generals, and mistresses. This lack of interest in the history of science has begun to disappear, partly as a result of a changing emphasis of history that is taking cognizance of the importance of economic, social, and intellectual movements; partly as a result of the devoted labors of such scholars as Paul and Marie Tannery, Pierre Duhem, Thomas Heath, George Sarton, Aldo Mieli, George Urdang, Henry Sigerist, and their disciples. Even political history has had to succumb to the encroachment of science with the advent of the fissioning atom. As a result of this, the place of chemistry in the history of civilization is receiving attention and it is important that those writing history of chemistry recognize this fact. It will be necessary to write history of chemistry that is acceptable not only to chemists but to historians. This must be true particularly of those works that are intended as scholarly books and papers, but it would be desirable even in those works written in a popular vein. It is important that we not continue to pass on unfounded legends as truth and that we be sure that our sources of information are carefully documented.

History of chemistry has reached the point where it must accept the responsibilities of adulthood. No longer can it depend upon the reminiscences of aged investigators who grew up with the science. Not that we wish to dispense with the reminiscences of the aged worker and of the hobbyist—history has always benefited from the assistance of the amateur—but our science has reached the point where the approach of the professional historian is becoming necessary.

It is perhaps not incorrect to call Edgar Fahs Smith the patron saint of American historians of chemistry. His enthusiasm for the subject, his first-hand acquaintance with famous chemists, and his activities in the founding of the Division of the History of Chemistry all cause us to revere him. Yet in truth it must be said that Smith was guilty of some of the very things that we must attempt to avoid. We owe Smith a real debt of gratitude for his many publications in the history of chemistry, particularly in the history of American chemistry, but it is a continual aggravation to find tantalizing statements in his works which, because of his failure to cite sources, are pursued further only with the greatest difficulty (*1*). This is often the case with those who write with the spontaneous enthusiasm of amateurs and who recollect from memory. To be useful to the historian, unusual information must be carefully documented so that it can be easily searched out in new contextual studies.

Even more to be abhorred is the scientist who follows one criterion of objectivity in the laboratory and a different one when writing chemical history. Paul Walden comes to mind as a particular offender. Despite being a Nobel laureate in chemistry, he has shown in his historical writings a total lack of objectivity in his viewpoint, going out of his way to emphasize the work of German chemists and rationalizing away the importance of the work of Boyle, Lavoisier, and other non-German workers (*2*). Nationalism has no place in history just as it has no place in science, and the biased viewpoints of such writers are to be condemned by the readers of history and avoided by the writers of history.

Equally appalling as the introduction of nationalism is the introduction of "isms" of other sorts. I refer, for example, to a history of science recently

written by the English chemist, Stephen F. Mason. On the whole, his book is a praiseworthy work, but it is rather appalling to find the author reporting the work of Kammerer on salamander inheritance in straight-faced fashion (*3*). The falsification of data in Kammerer's case has been known for many years, and there is no excuse for putting it forth seriously in order to establish evidence favorable to the Lamarkian view of evolution now favored by Marxist genetics.

Still other types of writing that we can do without are certain worshipful biographies and company histories such as have appeared from time to time. No chemist is so flawless that his biographers must describe him in terms of adolescent hero worship. No self-respecting company should desire its history described in terms that might have originated in the advertising department.

Having made a case for the accurate and objective writing of history of chemistry we may well raise the question, "What can be done to promote the writing of better history of chemistry?" The answer, it seems to me, is a twofold one. First, we must take far greater pains than have been taken in the past to preserve the source materials that are necessary for accurate writing of history, and second, we must train scholars who are not only competent in chemistry but in history as well.

Let us deal for a few minutes with the preservation of source material. The historian of chemistry may expect that his best source of information will be the published papers of the chemists who played roles in the developments that he is studying. Such papers, however, become increasingly less useful when we utilize modern journals. The mass of research submitted for publication today has forced editors to demand the ultimate in condensation when reporting results. Consequently, published papers often afford little insight into the work and thought of the author. As we reach back into the past we find this less and less true. In the early research publications of prominent scientists we are delighted to find a great deal of extraneous material reflecting their thinking on the problem under investigation. The books of Joseph Priestley, for example, *Experiments and Observations on Different Kinds of Air,* are delightfully revealing of the naive and innocent mind of the preacher groping for the secrets of nature.

Of greater importance than published works are the personal papers and laboratory notebooks of the chemists involved in historic researches. Such notebooks, diaries, letters, and other personal writings can be of greater value than published articles. Unfortunately these personal items are rarely preserved. Those of us interested in the promulgation of the history of chemistry face an important task in bringing about the preservation of such historical items. It is going to be necessary for us to make chemists conscious of historical values and to cause them to save their papers for the study of future investigators. We face a formidable problem in this respect, because frequently the value of certain papers is not apparent and they are readily discarded by heirs or even by the scientists themselves. Much could be done to prevent this loss of significant material if repositories for scientific papers and personal effects could be established in this country. Such archival repositories could then make available such papers for the studies of interested historians.

A start in this direction has been made in the Smith Collection at the University of Pennsylvania. Here there are preserved not only books and papers of Edgar Fahs Smith, but also those of Charles Albert Browne and Tenney L. Davis. The Collection has indicated a willingness to accept the personal effects of other chemists.

It would seem desirable to develop similar repositories in other parts of the nation. By having regional collections there will be developed a greater consciousness of the history of chemistry and the importance of preserving its source materials than if all efforts are concentrated on one collection. But not only must we encourage the preservation of source material, we must see that it is used. The best way to assure its use is by stimulating a greater interest in the history of chemistry and thereby encouraging the study of the subject as a professional discipline.

In dealing with the second problem, that of training historians of chemistry, we may well raise the question, "Who is competent for the writing of chemical history?" The question turns out to be a rhetorical one since there is no simple answer. We may set up certain ideal qualifications, but experience shows that great history has often been written by persons failing to have these qualifications, while dull, unreadable history has been written by historical scholars. We can only admit that greatness in historical writing, like greatness in chemical research, is a trait that cannot be recognized beforehand. Nevertheless, there are certain things that we can do to develop background for historical writing in chemistry and hope that the seeds will fall on fertile soil.

The historian of chemistry should first of all be thoroughly grounded in chemistry and such related fields as mathematics and physics. A knowledge of biology is also desirable, particularly for anyone dealing with the history of organic and biological chemistry. It is further desirable that our historian

have experience in the field of chemical investigation so that he can understand at firsthand the trials and tribulations of scientific research. At the same time he should have a sound knowledge of history, particularly of the history that deals with the economic and intellectual activities of the period in which chemistry has flourished. He should have an understanding of historical methods and be sufficiently well versed in bibliography to be competent in locating his source material, evaluating it, and citing it. Finally, he must be a sufficiently good linguist to be able to read the languages in which his material is found. I would suggest English, French, and German as a minimum. A historian who is unable to deal with each of these languages would find himself seriously handicapped. For studies in certain time periods, other languages may be equally important. The same may be true of studies of chemical developments in certain geographic areas. For the student of very recent developments in chemistry, certainly Russian, and probably Italian, Spanish, or the languages of the Scandinavian countries, would be desirable linguistic abilities. For the student of alchemy, Latin would be a minimum essential, Arabic and Greek would be highly desirable, with Syriac and Hebrew as additional languages that might shed light upon the problems under investigation. Sanskrit and Chinese would be useful to the historian who wishes to deal with the origins of Indian and Chinese chemistry.

Added to this is competence in the writing of one's own language. One of the grave shortcomings of much historical scholarship, particularly in the last century, has been the inability to write in clear and readible fashion. Historical scholarship has resulted in weighty, factual presentations, completely lacking in appeal even for the scholars who are interested in the field under discussion. It hardly seems necessary that historical writing be pedantic; rather, it should be for a wide audience, as has been the case with the greatest writers of history. It is to be hoped that future historians of chemistry will aim toward the great tradition of historical writing that we see in Macauley, Prescott and Parkman, Motley and Bancroft, Fisk and Henry Adams, and, in more recent times, in Samuel Eliot Morison, Douglas Freeman, Charles Beard, and Allan Nevins. The prospective writer of history will do well to read avidly in the literary classics of the past, not for the sake of discovering a model to be slavishly followed, but in order to acquire a breadth of experience in the style of the greatest literary masters of historical writing.

In dealing with the subject of professional training, one may well ask what has been done in this country to develop centers for such training. To the best of my knowledge there are now three universities that offer a broad program of graduate instruction in the history of science. They are Cornell, Harvard, and Wisconsin. None of these schools offers a doctorate in the history of chemistry since it is felt that the history of a specific science represents an unduly narrow degree of specialization. To successfully pursue reasearch in the history of a specific science, one should have a broad background in the whole history of science and culture.

All three schools permit a high degree of concentration in chemistry if the candidate's interest lies in that area. At Wisconsin the candidate takes a joint major in chemistry and history of science. He thus undertakes graduate work with the same undergraduate preparation in chemistry that is expected of all graduate students in the department. He must pass preliminary examinations in three of the four fundamental branches of chemistry—inorganic, organic, analytical, and physical—as do all doctoral candidates. In the history of science he must complete course work at an advanced level in the history of ancient, medieval, and modern science, attend seminars, and take a comprehensive preliminary examination in the history of science, at which time he is also examined in the history of chemistry. He is also expected to do some advanced course work in history, particularly intellectual history. The program is a rigorous one, but it should result in trainees who are competent to write and teach sound history of science.

A word may be in order regarding positions for such doctorates. As an academic discipline, history of science has not yet achieved sufficient respectability so that colleges are clamoring for their services. Consequently, the graduate may well expect to teach history of science in association with courses in elementary science. Our historian of chemistry is certainly qualified to teach elementary chemistry. He is superbly qualified to teach the physical science courses being offered in connection with the general education programs now being developed in many colleges. The two alumni of Wisconsin's joint major have found placement in such courses.

Despite the availability of professional training, I am sure that much history of chemistry will continue to be written by amateurs. The interest in history of chemistry is often late in its development, growing to full fruition only after the completion of formal education. More often, there is the fear that serious pursuit of historical studies will leave one at an economic disadvantage as compared to concentration on the more scientific aspects of chemistry. Thus we must face the problem of what the enthusiastic amateur can do to prepare himself for sound historical writing.

An important step is to become familiar with history of chemistry, history of science, and general history. There are many useful books in almost any good library. Guidance is available in Sarton's *Horus* (*4*), which contains extensive bibliographies on various aspects of the history of science.

General readings can be followed by the study of more detailed researches, such as those in *Chymia, Journal of Chemical Education, Isis, Annals of Science, Ambix, Science Progress, The Scientific Monthly, American Scientist, Archives internationales d'histoire des sciences,* and *Lychnos.*

There are various works on historical method (*5*) that provide valuable information regarding source materials, validation of data, organization of material, and writing itself. The progress of the latter is greatly aided by the availability of a good dictionary and a manual of style (*6*).

With such aids, plus personal enthusiasm and the same respect for objectivity in history that the chemist shows in the laboratory, there is no reason why important history cannot be written by the amateur.

Finally, it is important that all of us, whether professional or amateur, retain a wholesome sympathy for the reader. It is not enough to recite the facts of history, no matter how accurate they are. If written history is so repulsive that it remains unread, then it might as well remain unwritten. Let us approach the writing of history of chemistry in the spirit for which Samuel Eliot Morison pleads in his essay, "History as a literary art" (*7*).

In conclusion, it is evident that there is a real opportunity for those of us who are interested in the history of our science to extend this interest among both chemists and historians. For the chemist the discipline is important in showing the source of our present concepts and the growth of ideas. For the historian the discipline reveals and correlates the relationship of chemistry to intellectual, social, economic, and political developments. We have an obligation to make the discipline respectable by writing history of chemistry that is reliable, critical, properly documented, and interesting. We have a further obligation to encourage the preservation of source materials and to encourage the training of competent scholars in the field.

References and Notes

1. For example, see *J. Chem. Educ.* **2**, 548, 552, 555 (1925).
2. For examples, see Walden's *Drei Jahrtausende Chemie* (Limpert, Berlin, 1944), pp. 91, 99–101, 159, and *passim.*
3. S. F. Mason, *Main Currents of Scientific Thought* (Schuman, New York, 1953), p. 437. Also see the review of Robert C. Stauffer in *Isis* **45**, 201 (1954) and the letter of Conway Zirkle in *Science* **120**, 189 (1954).
4. G. Sarton, *Horus, a Guide to the History of Science* (Chronica, Botanica, Waltham, Mass., 1952). *Horus* is an extension of the author's *Study of the History of Science,* which, though superseded, is still valuable.
5. S. Kent, *Writing History* (Crofts, New York, 1941); A. Nevins, *The Gateway to History* (Heath, Boston, 1938); G. J. Garraghan, *A Guide to Historical Method* (Fordham Univ. Press, New York, 1946). J. T. Shotwell, *An Introduction to the History of History* (Columbia Univ. Press, New York, 1922); J. R. Strayer *et al,* Eds., *The Interpretation of History* (Princeton Univ. Pres, Princeton, N. J., 1943); C. L. Becker, *Everyman His Own Historian* (Crofts, New York, 1935).
6. There are various authors' manuals and writers' guides available. I have found Livia Appel, *Bibliographic Citation in the Social Sciences, a Handbook of Style* (Univ. of Wisconsin Press, Madison, 1940) particularly useful, and also the more comprehensive *A Manual of Style,* which is published by the University of Chicago Press. It is, of course, also necessary to know and follow the style of one's prospective publisher.
7. Originally published as one of the Old South Leaflets, Old South Meeting House, Boston. It is more readily available in Morison's *By Land and by Sea* (Knopf, New York, 1953), pp. 289–98.

In fact, it would seem that only those who had some first-hand experience in the acquisition of new knowledge in some disciplined field would be able truly to appreciate how great the science of the past has been, and would be able to measure those giant accomplishments against their own efforts to penetrate a few millimeters further into the darkness that surrounds them.—J. R. OPPENHEIMER.

Mendel and the Rediscovery of His Work

EDWARD O. DODSON

Dr. Dodson is associate professor of biology at the University of Notre Dame. He received his Ph.D. degree from the University of California, Berkeley, and his research interests lie in the closely related fields of cytology, evolution, and genetics. He is the author of a Textbook of Evolution *and will publish a genetics textbook in 1956. He has been fortunate enough to obtain a transcript of Mendeliana from the archives of Mendel's monastery and of the Augustinian order in Rome. This material was collected during the 1930's under the direction of Rev. Francis W. Howard, Bishop of Covington, Kentucky. It was entrusted by him to Sister M. Julitta of Villa Madonna College, who gave the material to Dr. Dodson. It is upon this material that this article is based.*

THERE are very few sciences for which a definite date of origin can be given. Genetics is one of these few, for, although men had wrestled with the problems of heredity since remote antiquity, no one had gained any insight until less than a century ago. Then one February evening in 1865, Gregor Mendel, a monk who had experimented with garden peas for 7 years, read a paper explaining his experiments before a small scientific society. And in that short paper were the elements of genetics, as we now know this discipline.

Mendel: Biographical Sketch

Gregor Johann Mendel was born in 1822 in the village of Heinzendorf, in Silesia (then a province of Austria but now in Czechoslovakia). The baptismal register of the parish church gives the date of his birth as 20 July, but the Mendel family celebrated his birthday on 22 July, and there is no way of establishing which date is correct. He was christened Johann. His parents, Anton and Rosina Mendel, were peasants who wrested a frugal living from a small farm, peasant-holding number 58 in Heinzendorf.

Education. Little is known of Mendel's childhood. He attended an elementary school under the tutelage of Thomas Makitta, who recognized the genius of Johann. The curriculum included natural science, a most unusual thing at the time, concerning which a school inspector wrote that "Pastor Schreiber is chiefly responsible for the growth of this scandal." Schreiber took a great interest in the schoolboys of his parish and spent much time with them. Also, he was interested in natural science and especially fruit culture. He spent much time instructing the local farmers in the grafting of fruit trees and otherwise improving farm methods. Anton Mendel was one of his most willing students, and he developed an excellent orchard. Thus Johann was, from earliest childhood, influenced in the direction of botanical and agricultural pursuits.

Upon graduation from elementary school, it was expected that Johann would work as his father's helper. But Makitta urged the Mendels to send their son to Leipnik for further schooling. His mother had always hoped that her son might rise above the peasant's status, but his father was reluctant. He yielded because of the hardships of the peasant's life: he was himself still required to work 3 days a week for the Countess Waldburg, under the old, feudal law of corvee. So Johann went to Leipnik, where he was always at or near the head of his class.

Having graduated from the school at Leipnik in 1834, he went to the district high school, or gymnasium, at Troppau, where he again distinguished himself. But school days were also days of great hardship for Johann, for the family funds were not adequate, and he was often hard pressed for the barest essentials of life. In 1838, his father was struck by a rolling log while he was working on the estate of the countess. His chest was crushed, and he was never again able to work his farm. Since further help from home was out of the question, Mendel took the course for school candidates and private teachers and, thus, was enabled to earn a frugal living by private teaching.

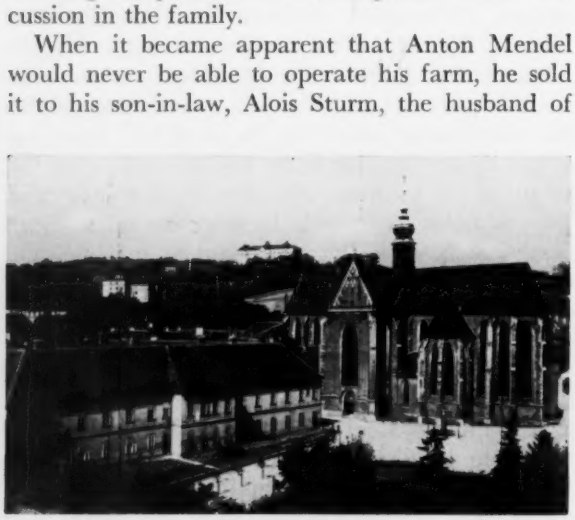
After his graduation from the gymnasium in 1840, Mendel wanted to continue his education at the Philosophical Institute at Olmütz. He tried to obtain a position as a private teacher in Olmütz but failed for lack of influential friends to recommend him. In his autobiography, he states that "sorrow over these deluded hopes and the dread-

fully sad outlook which the future held for him had so severe an effect upon him that he became sick, and was obliged to spend a year with his parents for recovery." He returned to Olmütz in the fall of 1841 and obtained enough work as a private teacher to take care of his minimal needs. At this point, his younger sister, Theresia, gave him her dowry in order to help with his education. How great a sacrifice this was may be realized when one remembers that at that time lack of a dowry seriously impaired a girl's chances for marriage. In his own time of prosperity, Mendel repaid her kindness, for he provided the education for Theresia's three sons, two of whom became physicians.

In spite of these improvements in his fortunes, the 2 years at Olmütz were still years of privation. He wrote in his autobiography that he "felt that it would not be possible for him to endure such exertions still further, and he saw himself forced therefore after the completion of his philosophical studies to enter a profession in which he would be freed of the bitter necessities of life. His circumstances determined his choice of profession. He applied and was received in the Augustinian monastery of St. Thomas in Altbrünn." And, so, in September 1843, Johann Mendel entered the monastery of St. Thomas, where he took the name Gregor, with which he always signed himself.

Because of the afore-quoted statement in Mendel's autobiography, some have doubted the sincerity of Mendel's religious profession. There is much evidence that he was not only sincere but was an exemplary member of the monastic community. His circumstances probably determined his final choice, but it is known that the possibility of his entering the priesthood had long been under discussion in the family.

When it became apparent that Anton Mendel would never be able to operate his farm, he sold it to his son-in-law, Alois Sturm, the husband of Mendel's older sister, Veronika. The sales contract included several provisions for Johann, including: "The purchaser shall pay to the son of the seller, Johann by name, if the latter as he now designs should enter the priesthood . . . the sum of 100 florins . . . and also annually, so long as Johann is still engaged in his studies, shall pay the father the sum of 10 florin convention coins as an aid to the cost of study, and shall also defray all the expenses connected with the first mass." One hundred florins was a pitifully inadequate subsidy for higher education, but it was the best that his family could provide. Had they been more prosperous, he might have become a parish priest rather than a monk.



The Augustinian monastery in Brünn.

While mulling over the problem of his circumstances and profession, Mendel sought the advice of Friedrich Franz, professor of physics at Olmütz, his most admired teacher and an Augustinian from Brünn. He had been asked to recommend candidates for the monastery from among his students, and he wrote that ". . . two candidates have presented themselves to me, of which I believe that I can only recommend one. This is Johann Mendel, born at Heinzendorf in Silesia, who in the two year course in philosophy has had almost invariably exceptional reports, and possesses a solid character. In my own subject, I can call him almost the most distinguished. . . ."

With such a recommendation it is little wonder that Mendel received a warm welcome from Cyril Napp, abbot and prelate of the monastery. Abbot Napp was himself a distinguished scholar, especially in the fields of oriental studies and the Old Testament. He had been zealous in the promotion of learning and special talents among his monks, with the result that the monastic community included outstanding scholars in the fields of botany, mathematics, physics, history, linguistics, music, philosophy, and theology. Thus Mendel went into a highly stimulating intellectual environment. His first year, a period of probation, was devoted to classical studies, but he wrote that his free time was devoted to the study of natural science. The following 4 years were devoted to the study of theology and allied subjects preparatory to the priesthood.

Mendel should not have been ordained until the conclusion of the fourth year of theological studies; however, the monastery was understaffed. Therefore, Abbot Napp applied for permission to have Mendel ordained at the end of his third year. In his letter, the abbot said that Mendel "has finished the third year of theology with praiseworthy success, has irreproachable conduct, and manifests a pious religious spirit." The Bishop of Brünn granted this permission, and Mendel was ordained a priest

on 6 August 1847. He was, of course, required to complete his theological studies. Meanwhile he gave service as an assistant pastor.

Mendel was not well suited to the work of a parish priest, for the sight of suffering made him ill. And so, when an opening for a substitute teacher became available in the gymnasium at nearby Znaim, he was relieved of his pastoral duties and sent to Znaim to begin his teaching career in the fall of 1849. There he taught mathematics and Greek. Mendel held this position for only 1 year, for, although the headmaster and the entire staff signed a most complimentary testimonial of the excellence of his service, he lacked the necessary credentials for a permanent appointment.

In the spring of 1850, Mendel applied for admission to the examination for teachers of natural science. As is well known, Mendel failed this examination, although one of the examiners, Count Baumgartner, was very favorably impressed. But another examiner, Kner, was equally unfavorably impressed. That he should have failed is hardly surprising, for he was entirely self-taught in natural science, and the examination was ordinarily taken upon the basis of university training.

Count Baumgartner, a very important man in science, business, and politics at the time, persuaded Abbot Napp to send Mendel to the University of Vienna for further training. There Mendel studied physics (under Doppler, the discoverer of Doppler's effect), chemistry, mathematics, zoology, botany, and microscopy during the years 1851–53. In the spring of 1854, he was appointed substitute teacher of natural science in the Brünn Modern School (an advanced technical high school).

In May 1856, he returned to Vienna to take the examination for his credentials, which would permit his appointment as a regular teacher. The records of the examination have been destroyed, but he did not pass. No certain information about the examination is available, but it seems unlikely that this candidate, whose name will surely outlive those of all his examiners, was not adequately prepared. There are some indications that he showed an unusual degree of independence of thought, and that the examiners took offense at his avid defense of theories contrary to their own. Some have thought that anticlericalism on the part of the examiners played a role.

Mendel never again tried to get his credentials, and for the entire 14 years of his service in the Brünn Modern School his title was merely that of substitute teacher (*Supplente*—often erroneously translated as *supply teacher*, a meaningless term), although he was on regular full-time duty. Mendel began his experiments with peas, which led to the discovery of the principles of heredity, soon after his return to Brünn, and school inspector Nowotny has expressed the opinion that these experiments were undertaken as a result of a dispute with the examiner for botany. If this is correct, then we may well owe the board of examiners a debt of gratitude for failing its most gifted candidate, and thus supplying the motivation for a program of research that has been extraordinarily fruitful.

Teacher and scientist. The years of Mendel's service in the Brünn Modern School were probably the happiest of his life. It is also the period of his life from which the best records of his personality are available. Although most of the available records of Mendel's life are formal, official documents, such as transcripts of credits, some of his former students have been interviewed or have written recollections of their much-beloved teacher.

Through these recollections there appears a man of reserve and humility but still a warm and human man, a man with great enthusiasm for his boys and for the subjects that he taught them. He was able to maintain discipline in his classes by the mildest methods, because of the fine friendly relationship that he maintained with his boys. He was skilled in making the most difficult material quite clear. "It was he," wrote Liznar, a distinguished meteorologist and one-time pupil of Mendel's, "who aroused in me the lust and love for natural science."

The schoolboys were welcome and frequent visitors at the monastery. There Mendel showed them his microscope and his telescope, his bees, and his flowers as well as the numerous tamed animals that he kept. He had a great love of animals. In the monastery garden, after picking some fruit for the boys, he would show them the technique of crossing plants, but he never hinted that he was himself engaged in original investigations along these lines. He was much more a friend than a master to his boys.

Mendel did most of his research during his teaching years. The basic experiments on heredity, which he published in 1866, were done at this time. Although he tried to continue such work in later years, when he was burdened with administrative duties, he gradually had to give it up. Yet his correspondence with the botanist Nägeli shows that he did try to corroborate his original experiments using other plants, especially the hawkweed, *Hieracium*. His interests were broad, and his experimentation ranged widely, although his fame rests on one series of experiments only. A second genetic paper was published in the proceedings of the Brünn Society in 1869 under the title, "On some *Hieracium* hybrids obtained by artificial fertiliza-

tion." He also published two papers on insects that are injurious to garden plants and at least three meteorological papers.

Mendel's interest in meteorology was profound, and he continued his studies in this field almost until the day of his death. He had a small telescope with which he kept a record of the sunspots. He made daily measurements on the level of subsoil water and also recorded other meteorological data. In addition to his own publications in this field, Liznar published on the basis of Mendel's data. Since Mendel was invited in 1881 to contribute data on the Brünn area to the Meteorological Institute of Utrecht, it is evident that his reputation in this field was international. Finally, Mendel was much interested in scientific bee culture, and it seems almost certain that he carried out genetic experiments with this difficult species, as he did with the mice which he raised. But of this work, no records are available.

Election as abbot. In the spring of 1868, Abbot Napp died, and Mendel's fellow-monks, by electing him abbot, conferred upon him the honor that the rest of the world had withheld. A preliminary election was held on 29 March, with 12 electors participating. Mendel received six votes, while the other six were distributed among four of his brother-monks. On 30 March, two ballots were cast. On the first, Mendel received five votes, his nearest competitor four, and three others one each. On the second ballot, Mendel received 11 votes, and thus the election was unanimous. His formal installation by the Bishop of Brünn took place on 13 April 1868, the first Monday after Easter.

On 4 May 1868, Mendel wrote to Nägeli that "recently there has been a completely unexpected turn in my affairs. On March 30 my unimportant self was elected life-long head, by the chapter of the monastery to which I belong. From the very modest position of teacher of experimental physics I thus find myself moved into a sphere in which much appears strange to me, and it will take some time and effort before I feel at home in it. This shall not prevent me from continuing the hybridization experiments of which I have become so fond; I even hope to be able to devote more time and attention to them, once I have become familiar with my new position." This, unfortunately, was a hope that could not be realized, for Mendel's new responsibilities took more and more of his time, so that his experiments had to be curtailed and finally abandoned, although he continued his meteorological observations until he was confined to bed by his final illness.

Mendel lived in close contact with his monks and took the greatest interest in all of them, including the novices. In later years, many of his monks were interviewed, and they all agreed that he had always been kind and considerate in ruling the monastery. Yet he was more zealous than his predecessor for the religious life of the community and for the strict observance of the monastic rule. A few, indeed, even left the monastery because of Mendel's insistence on the full observance of the Rule of St. Augustine and the other religious duties of the monastic community. Among his first acts as abbot were the restoration of the monastic church and the increase in quantity and quality of sacred vestments and vessels.

Unfortunately, Mendel's last years were overshadowed by a bitter controversy with the government, and this finally put an end to his experiments. In 1874, a law was enacted for the taxation of the monasteries. Mendel regarded the tax as unconstitutional and refused to pay it; at the same time he appealed to the government for a review of the constitutional question. The government refused to review the legality of the tax, and Mendel continued to his death to try to force such a review.

At first, all monasteries stood with Mendel, but one by one they capitulated before the power of the state, until he stood alone. He was offered many honors if he would give up the battle, and he was assured that, if he would only recognize the legality of the tax, the legal exemptions of the monastery would be sufficient to make payment insignificant or even to cancel it entirely. But, to Mendel, a question of principle was involved, and he felt that he could not in conscience yield.

To one raised in the American tradition, the whole controversy is difficult to understand. It seems self-evident that a prompt court hearing should have settled the constitutional question, as Mendel wished. But the Imperial Austrian government of that time took the viewpoint that it was beneath the dignity of the government to enter into a discussion of the validity of a law with an individual citizen. After Mendel's death, his successor obtained a settlement of the controversy by acknowledging the law and then applying for exemptions to such an extent that little or none of the disputed tax was payable.

Eichling's interview with Mendel. One of the best glimpses of Mendel's personality comes from this period. In 1942 C. W. Eichling, a flower-and-seed dealer of New Orleans, published in the *Journal of Heredity* his recollections of a visit with Mendel in 1878, when Eichling was a young man of 22 years. Eichling wrote as follows: "In the summer of 1878 I was traveling representative for Louis Roempler, one of the large novelty plant-and-seed firms of Nancy, France. . . .

"Not one of the leaders in the horticultural industry interviewed that summer mentioned the name of Mendel. Not until I reached Erfurt and called on Ernest Benary, the Nestor of European seed growers and breeders, did I hear Mendel's name mentioned . . . Benary mentioned Mendel . . . and his experiments with garden peas, *Pisum sativum*. I told Benary that Brünn was on my itinerary . . . and he suggested that I try to meet the Abbot if possible.

"On my arrival at Brünn I called on the only customer I had in the quaint old city of about 70,000 inhabitants. After our business was concluded, I asked about the Abbot Mendel. My customer stared at me in some astonishment and registered mild amazement at my inquiry. I was told that, while *der Herr Abt* was one of the best beloved clerics in Brünn, not a soul believed his experiments were anything more than a pastime, and his theories anything more than the maunderings of a charming putterer. . . .

"My first impression was a genuine and pleasant surprise. My customer's account had built in my mind a picture of an old, wrinkled, spooky, monk. Coming toward me was a fine looking, spectacled priest, smiling and extending a welcoming hand. His countenance expressed both determination and kindliness. I judged him to be about fifty years old. . . . having been told that German was the Abbot's favorite language we were soon in a lively conversation.

"I told Mendel that Ernest Benary had requested me to call on him and I discovered then that Mendel knew of nearly all the important men in the plant and seed line. . . .

"We then perused my catalogue together, and Mendel displayed great interest in the rare plants I had to offer and asked questions about their propagation. . . .

"It was now mid-day and the monastery bell called to dinner to which the Abbot invited me. A substantial meal, served in the Abbot's study, consisted of soup, home grown vegetables, home made bread, exquisite ham and beer. After a little chat, the Abbot invited me to walk with him through his extensive gardens. The grounds were kept as clean as a pin. I was shown beds of well grown vegetables and fruit trees of many varieties, each tree with a label. Mendel pointed with pride to a pair of espaliers against a wall, well loaded with fruit. . . .

"I repeat here that at the time I met Mendel I knew practically nothing of his discoveries and experiments and he forbore to enlighten me on the subject. He did show me several beds of green peas in full bearing which he said he had reshaped in

Gregor Johann Mendel. From a portrait hanging in the Moravian Mortgage Bank.

height as well as in type of fruit to serve his establishment to better advantage. I asked him how he did it and he replied. 'It is just a little trick, but there is a long story connected with it which would take too long to tell'. . . .

"I told Mendel that I had promised to make a report to Benary regarding these experiments, but Mendel changed the subject and asked me to inspect his hothouse. . . .

"Mendel asked me about my school days. . . . He was of the opinion that I should have gone through Heidelberg University. He became enthusiastic when he spoke of his own student years in Vienna and we even recalled some of our favorite student songs such as 'Gaudeamus igitur'—'Vom Hoh'n Olymp'—'Edite, bibite collegiales', etc. . . .

"My time to leave had arrived, and I thanked the Abbot for his great kindness to me, a young stranger. I knew from his hearty handshake and his blessing that I had made a friend. He accompanied me to the gate and made me promise to call on him again. This was not to be, because other plans prevented my returning.

"It is strange that when I asked Mendel about his work with the peas, he deliberately changed the subject. It could not have been that he had any desire to withhold the facts from me, for he had published the two immortal papers on his discoveries a dozen years before. It would seem that a direct invitation to talk about what must have been a very important episode in his life would have been eagerly seized upon. Could it be that this reticence was due to the complete oblivion his pub-

lications had achieved? Actually to have found the key to the riddle of heredity, and to have his discovery completely ignored both by his friends and by the scientific public must have been a tragic blow. . . . But the patronizing letter from Nägeli, advising him to spend his time more profitably, would have been enough to quench the enthusiasm of a very hardy soul indeed. Be that as it may, by failing to draw the master out with a sympathetic question I missed a priceless chance, in that garden sixty-four summers ago, to hear from the lips of the founder of Genetics how he made the discovery which today is recognized as marking an epoch in the study of life."

Illness and death. Mendel suffered from chronic kidney trouble for several years. In the fall of 1883, this became severe, and he also suffered from heart trouble. On the morning of 4 January 1884 he made meteorological observations, but on that same day he took a turn for the worse from which he did not recover. He died on the morning of 6 January 1884. His death certificate states that death was caused by heart failure and Bright's disease. Mendel had previously expressed a wish that an autopsy be performed after his death, and this was done by Brenner, with Mendel's nephew, Alois Schindler, observing. No pathology was found other than inflammation of the kidneys and an enlarged heart. After a funeral mass in the monastery church on 9 January, the founder of genetics was buried in the monastery cemetery. His funeral was attended by the dignitaries of both church and state but also by a great throng that included students who honored the memory of an inspiring teacher, poor people who mourned, as Iltis has said, "a true disciple of Christ, who knew how to give alms and not to humiliate the receiver," but there were none present who suspected that they were burying a great scientist.

Rediscovery of Mendel and Mendelism

Abbot Barina, one of Mendel's successors who served under Mendel as a young man, has quoted Mendel as saying with regard to his experiments in heredity, "Mein Zeit wird schon kommen"—"My time will surely come." It came in the year 1900, and the story of the discovery of Mendel and his long-delayed recognition, achieved through the efforts of three men working in ignorance of one another, is one of high drama, seldom equaled in the history of science. These three men, who, almost simultaneously, found Mendel's papers and recognized their worth, were Hugo de Vries, Carl Correns, and Erich von Tschermak.

De Vries was a distinguished Dutch botanist who had for many years been investigating problems of evolution in plants, and this had led him to make crosses between related species and varieties. In March 1900, he published two reports of this work. The first was a very brief note published in French in *Comptes Rendus de l' Academie des Sciences.* There he enumerated 11 monohybrid crosses that he had made using 10 different genera of plants. He explained the existence of "dominant" and "recessive" characters and their regular ratios of inheritance, and from these he derived the law of segregation. Although the technical terms that he used were original with Mendel, de Vries did not mention the name of Mendel in this paper; but in his second paper, a somewhat longer one of seven pages, published in the same month in the *Reports of the German Botanical Society,* he gives Mendel full credit for the discovery of the laws of dominance and segregation. The law of independent assortment was also presented in this paper, but de Vries seemed not to realize that Mendel's original work had also included this law, for he did not acknowledge it.

De Vries sent a copy of his first paper to Carl Correns, a young botanist at Tübingen University, Germany, who had been working on hybridization experiments in corn and in peas. He recognized in de Vries' paper the same results that he had himself obtained, and so he prepared a short résumé of his experiments for publication in the *Berichte der Deutschen Botanischen Gesellschaft.* This paper, published in April 1900, includes the statement that "the same thing happened to me which now seems to be happening to de Vries: I thought that I had found *something new. But then I convinced myself that the Abbot Gregor Mendel in Brünn, had, during the sixties, not only obtained the same results through extensive experiments with peas, which lasted for many years, as did de Vries and I, but had also given exactly the same explanation, as far as that was possible in 1866."*

It must be remembered that only the most elementary facts of cytology were known in 1866, neither mitosis, nor meiosis, nor even the chromosomes themselves having yet been discovered. In the main, the content of Correns' paper is similar to the papers of de Vries, but Correns' found cases in which hybrids were intermediate between the parental types, just as Mendel had; so he pointed out that de Vries "law" of dominance was not universally applicable.

At this point a little digression may be worth while. One of the famous incidents in the history of science concerns Wilhelm Pfeffer, a botanist at Tübingen and a pioneer of plant physiology. In the course of his studies on the effects of salt solutions on plant cells, Pfeffer discovered the phe-

nomenon of osmosis. He realized at once that this was more a matter of physics than of botany; hence, he tried to discuss it with a distinguished professor of physics. The latter turned a deaf ear, saying that physical principles were not to be discovered by botanists.

One might have expected such a rebuff to fill a young man with humility and receptiveness to new ideas for the rest of his life, but such was not the case. Many years later, the young Correns was a member of Pfeffer's staff at the time of the rediscovery of Mendelism. On the basis of his preliminary results, Correns asked Pfeffer for additional garden space in which to carry out further experiments on Mendelian heredity. But Pfeffer rebuffed him with the rejoinder that this was mere horticulture, unworthy of a botanist. Thus even great scientists may fail to learn their lessons!

In March 1900, both de Vries and Correns sent copies of de Vries' first paper to Erich von Tschermak, a young plant breeder who was engaged in postdoctoral studies at the Agricultural Institute of Vienna. He recognized at once the same results that he had obtained in the course of studies on peas during the past 2 years; in fact they had been the subject of a postdoctoral thesis that he had submitted to the Agricultural Institute on 17 January 1900. This long thesis was scheduled to be published in the course of time by the Agricultural Institute, but von Tschermak now felt that he should get it published more rapidly, and he submitted it to an agricultural journal that was able to promise more prompt publication.

"While going through the second correction of my own paper," writes von Tschermak, "there appeared, I may say to my horror, the paper of Correns. . . . It was now high time for me to come out with my part in the rediscovery of Mendel's laws. I therefore prepared an abstract of my extensive paper immediately for the June issue of the *Reports of the German Botanical Society*. . . . The three rediscoverers were fully aware of the fact that the independent discovery of the laws of heredity in 1900 was far from being the accomplishment that it had been in Mendel's time since it was made considerably easier by the work which had appeared in the interval, especially the cytological researches of Hertwig and Strasburger. Consequently the three rediscoverers were less interested in being celebrated as rediscoverers of rules they themselves designated 'Mendel's laws' than in the successful utilization of these laws for the development of their various fields: de Vries for the mutation theory, Correns for fundamental research in inheritance, and I for practical plant breeding."

Once this famous triumvirate had brought Mendel's work before the attention of the biological public, the new science of genetics developed, not without opposition, but steadily and even rapidly, to achieve a central position in the biological sciences, both pure and applied.

Why Was Mendel's Recognition So Long Delayed?

The question of why the recognition of the value of Mendel's work was so long delayed has vexed the historians of science. It used to be standard to answer this question with the statement that Mendel published in an obscure journal that simply was not seen by those scientists who might have appreciated its worth. This cannot cover the case, for recent research has shown that the journal was in the libraries of most of the universities of Europe and America. Yet it may well be that busy scientists, who did not have time to cover adequately all the important journals at their disposal, simply did not read one in which they did not expect to find anything significant. Von Tschermak mentions that Mendel sent a reprint of his paper to the Viennese botanist Kerner, who did not even cut the pages of the reprint.

This plaque in the monastery garden, inscribed in Czech, German, French, and English, commemorates Mendel's work with the statement: "Prelate Gregor Mendel has made experiments for his law here."

Statue of Mendel at Brünn, by Theodor Charlemont.

Mendel's correspondence with Nägeli. Mendel's work was well known to at least one botanist who ought to have appreciated it fully: Karl Nägeli, professor of botany at Tübingen University, a specialist in the field of hybridization, and often regarded as the most distinguished botanist of his time. Mendel sent Nägeli a reprint of his paper and invited Nägeli's suggestions, especially with respect to plans for similar investigation of the genus *Hieracium* (hawkweed), a plant on which Nägeli had done much work. Nägeli replied in condescending tone, saying, with respect to the pea experiments, that they "are not completed but that they should really just begin," a severe stricture, considering that Mendel's published report was based on more than 10,000 plants. But he did suggest willingness to repeat the experiments if Mendel could provide him with suitable seed. Mendel eagerly sent him no less than 140 packages of seeds, each labeled with full information regarding ancestry and recommended use in experiments. Some of these seeds were planted in the spring of 1867, but no experiments were carried to completion, and apparently Nägeli made no further reference to Mendel's experiments with peas, although their correspondence continued until 1873.

Mendel's letters to Nägeli have been preserved by Correns, a one-time student of Nägeli. To this we owe the knowledge that Mendel, in addition to the work with peas and beans, planned or performed similar experiments with no less than 18 additional genera! But the letters are principally concerned with the *Hieracium* crosses. In these, Mendel found a partial confirmation of his major work, but he also found puzzling exceptions that he could not understand. Today the answer is simple: *Hieracium* is an *apogametic* plant; that is, it reproduces readily by development of unfertilized ovules as well as by the more typical method. In Mendel's time, the basic data for the understanding of this phenomenon were not yet available. But the Mendelian laws depend on normal, biparental reproduction for their operation.

Mendel and Darwin. Early in the present century, Bateson suggested that the history of genetics would have been very different had Darwin been familiar with Mendel's papers, for Darwin was thoroughly familiar with the literature on hybridization, he had himself wrestled with the problem of heredity, and he realized that the weakest aspect of his own theory of the origin of species lay in the lack of reliable information on the fundamental principles of heredity. Bateson believed that Darwin would have recognized the value of Mendel's work, and that he would have obtained for Mendel the recognition that he deserved.

That Darwin did not come across Mendel's papers seems practically certain, for his library has been thoroughly searched. Darwin was an inveterate scribbler of marginal notes, yet none of his personal books contain any references at all to Mendel, nor are Mendel's own papers to be found there. While it may well be that Bateson was right, still the thesis is unprovable, and it may very well be wrong, for surely the same thing would have been said of Nägeli were the history of his relationship to Mendel unknown.

It may also be noted that Mendel's mathematical treatment of his botanical data must have seemed strange in that time, when quantitative biology was unheard of.

Whatever the real cause may have been, there have surely been few if any discoveries of comparable magnitude that have been so completely ignored in the time of the discoverer. In only three known works was Mendel cited at all before the rediscovery by de Vries, Correns, and von Tschermak. The first of these was a book dealing with evolution that was published in 1869 by the German botanist Herman Hoffman. But, like Nägeli, he missed the real point. Second, W. O. Focke in 1881 published a comprehensive bibliography of plant hybridization (*Die Pflanzenmischlinge*), in which he referred to Mendel's work and listed him as one of the most reliable observers. It was through this reference that all three of the rediscoverers found that Mendel had priority over them by 35 years. Finally, the American botanist Liberty Hyde Bailey

cited Mendel in a paper published in 1892. Insignificant papers commonly get much more attention than that. But Mendel said that "my time will surely come"; it did come in the spring of 1900, and it will last as long as mankind honors its benefactors.

Genetics, a young science. In concluding this historical survey, it may be profitable to emphasize the fact that these events are recent ones. At the time of this writing, there is still at least one man living who talked with Mendel (C. W. Eichling). Of the three residscoverers, Erich von Tschermak-Seysenegg is still living (Seysenegg, an ancestral name was added to the family name in 1907 when Erich's father, a distinguished geologist, was ennobled). Throughout his long life, he has fulfilled the high promise of his youth, having been one of the most productive of the Mendelian plant breeders.

In 1950, the Genetics Society of America sponsored a program in honor of the Golden Jubilee of Genetics. Five of the participants in the 3-day program were among those who were the pioneers of the new science of genetics at the turn of the century. And another half-dozen were what might be called first-generation geneticists—they entered the field somewhat later, after it was well established, but took their training from the pioneers. Thus there are among us today many living links with the first days of genetics.

Notes and Bibliography

* Mendel's autobiography was written in the third person, as a part of his application for admission to the examination for high-school teachers in 1850.

L. C. Dunn, Ed., *Genetics in the 20th century* (Macmillan, New York, 1951).

C. W. Eichling, "I talked with Mendel," *J. Heredity*, **33**, 243 (1942).

H. Iltis, *Life of Mendel* (Norton, New York, 1932).

G. Mendel. "Versuche uber Phlanzen-Hybriden." Reprinted as a facsimile reproduction of the original in *J. Heredity*, **42**, (1866). In translation, *Experiments in plant Hybridization* (Harvard Univ. Press, Cambridge, Mass., 1948).

C. Stern, Ed., The birth of genetics. Suppl. to *Genetics* **35** (No. 5, pt. 2), 1–47 (1950).

E. Von Tschermak-Seysenegg, "The rediscovery of Gregor Mendel's work," *J. Heredity* 42, 163 (1951).

Scientists must be independent. The pursuit of science can be organized, therefore, in no other manner than by granting complete independence to all mature scientists. They will then distribute themselves over the whole field of possible discoveries, each applying his own ability to the task that appears most profitable to him. Thus, as many trails as possible will be covered, and science will penetrate most rapidly in every direction toward that kind of hidden knowledge which is unsuspected by all but its discoverer, the kind of new knowledge on which the progress of science truly depends. The function of public authorities is not to plan research, but only to provide opportunities for its pursuit. All that they have to do is to provide facilities for every good scientist to follow his own interests in science. To do less is to neglect the progress of science; to do more is to waste public money. Such principles have in fact essentially guided all well-conducted universities throughout the modern age.—MICHAEL POLANYI, *Bulletin of the Atomic Scientists* **5**, 20 (1949).

How Does Rock Break?

FRED C. BOND

Mr. Bond, who is a metallurgical engineer, is associated with the Allis-Chalmers Manufacturing Company, West Allis, Wisconsin. Since his graduation from the Colorado School of Mines, he has also worked for the New York and Honduras Rosario Mining Company, Tennessee Copper Corporation, Allis-Chalmers Basic Industries Research Laboratory, Eldorado Gold Mines, Ltd., and Compania Minera Nacional of Peru. He is the originator of the third theory of comminution and has written numerous technical papers on grinding and crushing of ore and mineral dressing.

APPROXIMATELY 1 billion tons of rock, ores, cement clinker, road stone, and other materials are crushed and ground each year in this country. This is enough to build a roadbed 100 ft wide and 15 ft high from New York to Los Angeles. The power used and the steel worn away and discarded in crushing and grinding each approach 1 percent of our total national production. The principal item in the operating cost per ton of grinding is the mechanical energy requirement; the next major item is the cost of the metal worn away and discarded. The metal-wear cost is closely related to the power cost, so that any estimation of over-all expense requires a knowledge of the energy input required. Rock-size reduction is one of the large items in cost of metals, roads, and buildings.

From the beginning of Paleolithic time, rock working has been an essential of human culture, yet we still know very little about the actual mechanism of the process.

In 1867 P. R. von Rittinger (*1*) stated in Berlin his theory that the work done in crushing and grinding is directly proportional to the new surface area produced. This seemed reasonable and was widely accepted. However, Friedrich Kick (*2*) disagreed in Leipzig in 1885; he thought the work required varied inversely as the volumes of the particles produced and cited stress-strain diagrams to prove his point. Acording to Rittinger, the work required to break one particle varies as its diameter squared, and the work required per ton varies inversely as the average diameter of the product particles. According to Kick, the work required per particle varies as its diameter cubed, and the work required per ton for any given ratio of feed size to product size remains constant for all product sizes.

The controversy continued for well over two generations, with the Rittinger adherents gradually gaining the upper hand. They developed various methods for measuring or estimating surface areas of finely ground materials and found some indirect methods for evaluating the very small surface energy of solids. Tests made with feed particles of one size seemed to confirm the surface area theory. However, they could never explain away the Kick premise that work equals force times distance, since Rittinger made no allowance for the distance of deformation before breakage.

Although the amount of rock broken increased year by year, the two theories continued with very little practical application. Kick's theory seemed to hold fairly well in some coarse crushing tests where few fines were formed; Rittinger's was better for fine grinding but seemed to exaggerate the importance of the extreme fines. Industrial installations amounting to many millions of dollars were compelled to use empirical rule-of-thumb calculation methods.

The third hypothesis (*3*) was first announced in 1951. It was based upon a concept of the process of brittle breakage, and used the crack length formed in place of Rittinger's surface area or Kick's particle volume. According to this hypothesis, rock breakage is produced by deforming the rock, commonly under pressure, until the resulting stress locally exceeds the breaking strength and a crack tip forms, usually on the surface. The surrounding strain energy then flows to the new crack, which is thereby extended to split the rock. When the rock breaks, or the strain is otherwise released, the mechanical energy input is transformed into heat. A crusher or grinding machine thus becomes a device for transforming mechanical energy into heat under conditions that promote material breakage; and the absolute mechanical efficiency of comminution becomes a very evanescent quantity.

The third hypothesis work input varies as the crack length formed, or as one-half of the square root of the new surface area produced. The work per particle varies as the diameter to the 5/2 power, which is intermediate between the Rittinger and the Kick exponents; and the work required per ton varies inversely as the square root of the diameter of the product particles.

The designation of the three opposing explanations of rock breakage illustrates a peculiar difficulty in the presentation of new ideas. Many years ago, the unopposed Rittinger explanation was called the Rittinger *law*. Then the Kick proponents did not wish to place their explanation on a lower level of probability than the Rittinger and consequently it became known as the Kick *law*. I believed that the third explanation was more probable than the other two; but since three opposing laws are more than a little ridiculous, I demoted them all to theories. However, the third explanation is so new that on the basis of chronology and accumulated scientific evidence it is entitled to no higher rating than that of *hypothesis,* which is assigned to it here.

Perhaps all new explanations of physical phenomena should start life in a hypothetical kindergarten and should be advanced to theories only at adolescence, and to laws at evident and powerful maturity. In our changing world the future may belong to the hypotheses.

In applying the third hypothesis, the square screen hole through which 80 percent of the rock would pass is used to define the size of crushed and ground materials, and this is found by plotting size distribution screen analyses. The feed size F is the diameter in microns which 80 percent of the feed passes, and P is the micron size through which 80 percent of the product passes. One inch is 25,400 μ. The kilowatt hours of energy input required per short ton is designated as W, and Wi is the work-index, or the kilowatt hours per ton required to reduce from a theoretically infinite feed size to 80 percent passing 100 μ, which is equivalent to about 67 percent passing a 200-mesh screen with 74-μ openings.

The work-index is a practical yardstick for measurement and comparison of the energy input required in commercial reduction machines, including their average friction losses and motor efficiencies. Development of an exact net work-index as a specific physical property of rocks must await a determination of the absolute mechanical efficiency of comminution.

The basic third hypothesis equation is

$$W = 10\,Wi/\sqrt{P} - 10\,Wi/\sqrt{F}$$

The work-index of any material can be determined by laboratory crushing and grinding tests, and it can be found for any commercial crushing or grinding installation where the feed size, product size, and energy input per ton are known. Efficiencies can be compared, and machine sizes for new installations can be selected from the energy input calculated by the foregoing equation. A few average work-index values are listed in Table 1 for dry crushing and wet grinding. Dry grinding requires about 4/3 as much energy as wet grinding.

Table 1. Some average work-index values.

Material	Work-index (Wi)
Cement clinker	13.6
Cement raw material	10.6
Clay	6.3
Coal	13.0
Copper ore	12.4
Flint	26.0
Gold ore	14.9
Granite	15.1
Gravel	16.1
Iron ore—taconite	14.6
Limestone	12.5
Oil shale	15.8
Quartz	13.6
Slag	10.6
Trap rock	19.3

The third hypothesis of comminution definitely follows the facts of commercial crushing and grinding more than the other two theories do as well as having a practical application in such cases. It provides the work-index as a common measuring stick of the work required for any material over any size range of feed and product. It permits more accurate estimates on new installations and better computations of relative efficiencies.

However, our method of breaking rock is fundamentally the same as that of the prehistoric arrowsmith. We still expend large amounts of mechanical energy in deforming the stone and producing a crack, the energy being dissipated as heat. A scientific search for an easier and more economical method is indicated.

Rock and other crystallized materials, including metals, seem to have a fine microstructure; irregular zones of weakness interrupt the regular lattice-spacing of each crystal in mosaic blocks at intervals of about colloidal dimensions. These divisions in rock are approximately 0.7 μ apart, and this size is called the *grind limit,* since reduction to particles below this size requires a large increase in energy input. It seems likely that ordinary grinding is concerned with breaking rock along these microstructure zones and along crystal boundaries rather than with the disruption of perfect crystal lattices. The work required by the third hypothesis equation to reduce quartz to the grind limit is less than half of that required to melt the quartz, but reduction to molecular dimensions requires many times the energy necessary for melting.

If the energy required for breakage by deformation can be substantially reduced in any manner, it

Spider assembly of a gyratory crusher. This machine crushes hard granite, which is discharged at 5-in. size. Its average capacity is 350 tons/hr, and its maximum capacity is 500 tons/hr.

appears probable that the reduction will be made by applying force at selected sites. Reduction is now accomplished by undirected brute force applied at protruding points. The force applied at each contact can be only roughly adjusted to the average size and hardness of the particle contacted.

It is possible with the new superhard cutting materials available that force may be applied to the larger rock fragments by sharp edges or points, whereby particles of the desired size may be obtained with less energy expended and with a reduced production of fines. The main question here is simply whether the cutting edges can be kept sharp.

It is also possible that vibrational energy can be so synchronized with the velocity of propagation of a shock wave within the crystalline or mosaic microstructure of the rock that splitting can be accomplished by directing force at vibrational nodes with relatively small energy input. This is one of the intriguing possibilities of the future.

Some method of obtaining comminution by a directed energy flow may revolutionize the breaking of rock and result in a great saving of power and steel. This would make available lower grade ores for exploitation and thereby reduce the cost of metals, roads, and building construction—with a corresponding increase in the per capita wealth.

References and Notes

1. P. R. von Rittinger, *Lehrbuch der Aufbereitungskunde* (Berlin, 1867).
2. F. Kick, *Das Gesetz der proportionalem Widerstand und Seine Anwendung* (Leipzig, 1885).
3. F. C. Bond, "The third theory of comminution." *Trans. Am. Inst. Mining Met. Engrs.* **193**, 484 (1952); *Mining Eng.* (May 1952).

J. J. Thomson was more than a great pioneer of science; he was a great teacher and an outstanding personality. If it was the fame of his scientific gifts which attracted to him at the Cavendish Laboratory students from every part of the civilized world, it was his personality which held them, and drew from them an affectionate reverence which was little less than worship. To them he was always "J. J.," and there was no one like him, and to them he devoted himself unsparingly. It was Thomson's deep conviction that the highest function of a university research department was not the multiplication of discoveries, but the training of men. In the universities and research departments of many lands, men trained by him in the arts of experimental research still carry on, in their various ways, the Cavendish tradition.—Journal of Scientific Instruments **17**, 217 (1940).

BOOK REVIEWS

Cultural Anthropology. An abridged revision of *Man and His Works.* Melville J. Herskovits. Knopf, New York 22, 1955. xiii + 569 + xxxiii pp. Illus. + plates. $5.

It was not very long ago that textbooks were rather rare in anthropology. Now each year sees the addition of several titles to the list of introductory treatments, and the present volume marks the initiation of the "new and revised" edition in this discipline. In 1948 Herskovits published *Man and His Works,* a textbook covering the "four fields" of anthropology. The book seems to have had considerable initial success but later declined in popularity because of its length and difficult style. A condensation was in order, and the result is *Cultural Anthropology.*

Though the book is called an "abridged revision" of its predecessor, the revision aspects are largely matters of organization, style, and the addition of references. A few obvious changes have been made in the substantive material. Herskovits, taking his over-all orientation in human biological evolution from Weidenreich, carefully set aside Piltdown in 1948 as dubious. In *Cultural Anthropology* the fraud is made explicit. Another example of the extent of revision is the minor rewriting of certain sentences on cultural relativism (p. 364), following Hoebel's criticism of the early edition [*Am. Anthropologist* **51,** 473 (1949)]. Yet certain concluding remarks that drew the identical criticism (1948 ed., p. 653) stand unchanged in the present version (p. 544–545).

The major value of Herskovits' book to me lies in the broad coverage of significant points of view that have developed in cultural anthropology within the past few decades. The general reader can learn a great deal about the major currents of thought in this discipline from the book, but a caveat must be issued. Although Herskovits seems eclectic in his approach, there is a basic and personal view that runs through the whole work. This may be identified as a relativistic, antievolutionary, antideterministic position that often colors the author's approach to specific problems. Furthermore, comparison of the original and revised editions reveals no significant shift in any opinion held by Herskovits during the 7 years between the two. Instead, the additional materials, for the most part references appearing in the footnotes, add to the documentation of points already made. A few examples may be given. Herskovits continues to neglect the Australopithecines, mentioning only one advanced form, *H. njarasensis,* but neglecting the exciting materials of earlier date and more primitive morphology. In the too brief treatment of the Neolithic and terminal Paleolithic, no mention is made of the revelations supplied by the recent work of Braidwood and others in Iraq and Iran. Nor is there any reference to the problem of agricultural origins in southern or southeastern Asia.

Finally, in dealing with culture, per se, there is no treatment of the concept of the symbol, although the word *symbolism* appears several times. The lack of this analysis leaves Herskovits in a most awkward position in his discussion of the distinction between culture and society (p. 317):

> Social institutions may be broadly interpreted to include economic and political orientations as well as those based on kinship and free association. But only with difficulty can they be extended to include religion, the arts, and language, to say nothing of the unspoken sanctions that underlie all conduct.

Such a passage is all the more amazing because, earlier, Herskovits has said of Durkheim's analysis of religion that, although it "must be substantially modified, it is by no means to be completely dismissed" (p. 229).

Individual instructors will use or reject this book, now that so many other textbooks are available, in accordance with the needs of their courses. Readers who are not professionals will profit from this volume but should not make it the sole source of their impression of the character of modern anthropology.

MORTON H. FRIED
Department of Anthropology, Columbia University

The Evolution of an Insect Society. Derek Wragge Morley. Scribner, New York, 1954. vi + 215 pp. Plates. $3.95.

This is a fascinating book, even to those biological novices who can do no more than recognize an ant as such when they see it scurrying by. Its main theme is the evolution of a highly social (not to say communistic) mode of life in this group of insects.

The author begins by describing in detail a wood ant commune somewhere in Surrey, England. Here live almost a million ants in a number of interconnected communities. Throughout the warmer months of the year the denizens of the commune engage in activities so varied that it is very tempting to compare them with those of human society: there are, of course, the queens, males, and workers, but among the last some are foragers, some are builders, and there are others to wait upon the queen, to nurse the grubs, and even to act as sentinels at the gates, after these are closed at nightfall. Sometimes the commune is invaded by enemies, such as the slave-making ants, or there may even be civil war. Then the residents engage in combat, often to the death. Recognition of friend or foe, or of familiar and unfamiliar territory, is generally by smell, perceived through the antennae. As in human society, some individuals are cleverer than others, and they can learn from experience to a degree. Such individuals excite others to engage in activities similar to their own; if they are removed from a colony its activities are for the time considerably limited.

There are few criticisms of the book. The author has been a student of ant life for many years, and is obviously an authority. One may raise the question whether some details of his account of the evolution of wood ants are warranted in view of the paucity of fossil evidence (the earliest fossil ants are of Eocene age). Also, physiologists will probably be skeptical of such statements, occasionally occurring in the discussion of the foundations of ant behavior, as ". . . neurons, which are constantly moving . . . by means of a pumping mechanism. . . ." Nevertheless, the book is, in general, well written, well illustrated, and well worth reading.

REGINALD D. MANWELL
Department of Zoology, Syracuse University

Readings in Anthropology. E. Adamson Hoebel, Jesse D. Jennings, and Elmer R. Smith, Eds. McGraw-Hill, New York–London, 1955. xiv + 417 pp. Illus. Cloth, $5; paper, $3.75.

Books of readings can run the gamut from a patchwork of scraps and morsels to a symphonic blend of interweaving melodies. A patchwork may contain the more stimulating readings because each of its items can be chosen for its individual interest or distinction. In anthropology, moreover, isolated readings are easier to obtain than integrated effects, and this is paradoxical, for the special goal of anthropology is to see all human ways of life as integrated affairs, as more than the sums of their parts. This may be a goal, but it is neither a method nor an accomplished fact. No single discipline can study all of human life in both its complexity and unity. Human biology must be geneticized, speech phonemicized, and psyches psychoanalyzed. Thus, in spite of its goal, the study of man is rarely an integrated affair itself; and a book of readings in anthropology is often a patchwork.

Hoebel, Jennings, and Smith, however, strive for a total effect. They choose the parts for the whole. Accordingly, their readings do not include a single selection from such well-known names as Boas, Kroeber, Sapir, Benedict, Mead, Tylor, and others. The 52 selections that the editors have chosen "represent *anthropology,* not *anthropologists.*" They present these selections without editorial introductions, because, they say, "each piece speaks for itself, and each relates to the others with a smoothness and effectiveness that we had not expected at the outset." The readings are simply arranged under eight headings. What then is the total effect?

The eight parts follow one another well. Unfortunately, however, there is little connection between the readings in prehistory, those in primitive technology, or those in physical anthropology. The readings within each of the parts also follow one another well, and as a group they provide a good introduction to various parts of the subject. The part on linguistics, however, contains only two selections, and they are quite unrelated. The selections on primitive culture, culture and society, and applied anthropology provide both data and interpretations that reveal the complexities and the unities of culture. Individually the readings are significant, and collectively they are as coherent as the subject permits. Students and teachers will welcome this anthropological anthology for the quality of its selections. It provides a foundation and a stimulus for further study.

ALFRED G. SMITH
Department of Sociology and Anthropology, Antioch College

Animal Life in Deserts. P. A. Buxton. St. Martin, New York, ed. 2, 1955. xv + 176 pp. Illus. + plates. $4.25.

This is a reprint of the original 1923 edition, essentially a facsimile. While the publishers state that a number of errata have been corrected these are evidently of such minor character as to have escaped notice in a page by page comparison of the first 90 pages and sampling of the remainder. The plates are substantially as clear as in the first printing.

Although 35 years have elapsed since the original publication, no better general introduction to the subject has yet appeared in English. Much detailed work on the desert as a habitat and on the physiology of desert organisms has been done in the meantime, largely in the way of elaboration of principles assembled by Buxton. Some of it will appear in the forthcoming report of the Arid Lands Conference recently held in New Mexico, scheduled to be published as a AAAS monograph. Papers by Draz, Schmidt-Nielsen, Whyte, Beltran, and Uvarov will deal particularly with animal life in arid regions.

A general review of the first edition by William C. Cook in *Ecology* **5,** 415 (1924) does full justice to the present reissue.

PAUL B. SEARS
Conservation Program, Yale University

Evolution of the Vertebrates. Edwin H. Colbert. Wiley, New York; Chapman and Hall, London, 1955. xiii + 479 pp. Illus. $8.95.

This is a very well-written and carefully prepared review of vertebrate paleontology. The illustrations are all new or redrawn and are quite effective. The actual evolutionary history of the vertebrates is traced through geologic time, taking the classes and subordinate units in taxonomic order. In his preface, Colbert indicates that the book is aimed at the general student and the lay reader rather than at the specialist. The dual aim has led to the definition of some terms for the benefit of the lay reader, whereas others, equally technical, are not defined. However, any student with a knowledge of elementary zoology and comparative anatomy should have no trouble with understanding the text. The duality of purpose also shows up in the list of references, which would merely serve to direct students to sources where they could find other references. The book is practically perfect typographically, and factual errors, if any, are too trivial to mention.

There is (apparently intentionally) very little theoreti-

cal discussion of the factors involved in the evolution and differentiation of the vertebrates, of the problems of extinction, or of the problems of intercontinental migration, all of which might have proved stimulating to the student. In places, the students might get the impression that the life-span of a group of organisms was identical with its known geologic record. The one important criticism of the book is that there are no figure references in the text, so that the individual reader has to correlate the illustrations with the text. Although this might not be important to someone reading the book for relaxation, it complicates its use as a reference.

This book ably fills a definite gap in the literature of vertebrate paleontology, being suitable for use by advanced zoology or biology students who are not interested in becoming professional paleontologists.

ALBERT E. WOOD
Department of Biology, Amherst College

Grundriss der Allgemeinen Zoologie. Alfred Kuhn. Georg Thieme, Stuttgart, Germany, ed. 11, 1955 (order from Intercontinental Medical Book Corp., New York 16). vii + 281 pp. Illus. $3.95.

The present book is the principal introductory textbook in zoology used in German universities. Its success is attested by the fact that it has gone through 11 editions since it first appeared in 1922. It is, therefore, of interest to consider the way in which this book deals with the fundamental problems encountered in the teaching of elementary zoology. One of the main problems consists in the selection of subject matter out of the enormous wealth of facts. The older, well-established knowledge in zoology should be included in an introductory textbook but without presenting the student with an anachronistic picture of the science. At the same time, many of the present problems in the biological sciences require for their understanding a fairly advanced knowledge of chemistry and physics, which cannot be generally expected of a student in elementary biology. These problems have been solved in the present textbook by concentration on the parts of zoology that have a self-contained conceptual basis, such as comparative morphology, physiology with reference to structure, genetics, embryology, and evolution. Biochemical problems are frequently pointed out but are not elaborated and are left to more advanced courses.

A short introductory part deals with the general characteristics of life, the properties of protoplasm and the cell theory, and formulates the problems and fundamental concepts of the sciences. The second part consists of a systematic treatment of the structure of animals, both invertebrate and vertebrate. In this part, the major animal phyla are introduced not by the use of representative species but as types from which the individual organisms can be derived. An excellent example is the scheme of the organization of the phylum Mollusca (Fig. 64), from which on the following pages the molluscan classes are derived.

The third part, the activities of animals, deals with physiology. Here, again, the emphasis is on general principles rather than on the intensive study of particular organisms. Particular organisms are brought in as examples, and the examples are chosen both from vertebrate and invertebrate zoology. The classical knowledge in the field is thoroughly treated, but the recent developments and problems are everywhere emphasized.

The fourth part deals with the processes of development. In this part are included genetics, descriptive embryology, and the principles of experimental embryology. Two shorter parts deal with ecology and the theory of evolution.

Added are a list of books that the interested student may consult for further information, and an index. In the list of literature, books in English and French are included.

The great value of this book lies in the encyclopedic knowledge of the author, his ability to integrate the elements of this knowledge into a coherent scientific picture, and in his ability to express his thoughts briefly and with great precision. His formulation of definitions and of problems is always thought provoking and stimulating. The precision of the style enables the author to cover a vast amount of factual material in a limited amount of space. The educational value of the book is increased by the large number of excellent original illustrations. All of them are pen-and-ink drawings; some of them are schematic so as to illustrate particular points; others are very naturalistic. All of them are very clear and will be of great value to the student.

Owing to the vivid style and the clarity and brevity of diction, the book makes interesting and stimulating reading not only for the elementary student but also for the advanced biologist. The success of the book in stimulating the curosity of its users is attested by the large number of biologists who obtained their first introduction into the field of zoology from the earlier editions of this book.

ERNST CASPARI
Department of Biology, Wesleyan University

Introduction to Physical Geology. Chester R. Longwell and Richard Foster Flint. Wiley, New York; Chapman and Hall, London, 1955. x + 432 pp. Illus. $4.95.

The *Introduction to Physical Geology* by Longwell and Flint is a book that is new from title through the cover linings to the approach, organization, and illustrations. It is very evenly balanced and is remarkably free from undue emphases on the specialities of the authors. A refreshing change from the tendency of recent elementary books to be thick and expensive, it is shorter, more concise, and less expensive.

Influenced by a questionnaire sent to more than 100 college teachers, the authors have reorganized the order of presentation. The first four chapters present what geology is and does; the shape, size, and major features of the earth; the materials of the earth's crust; and the concept of geologic time and measurement in a manner that would seem light and personal were it not so pro-

found. Some of the subject matter is what in the past the teacher has had to supplement. Igneous geology as a primary feature of the earth is presented before weathering. The chapter on sedimentary rocks is deferred until after discussion of the fluid agents that deposit sediments. If this arrangement makes some people unhappy, they would have no difficulty in studying the chapters out of order.

In each chapter the authors have entered into the subject matter deeply enough to clarify major principles. Their aim has been to bring into focus a central theme of physical geology, the conflict of erosion, sedimentation, and uplift of the land, with repeated emphasis on the concept of geologic time. Physical, biologic, and geologic principles are stated and elaborated with a minimum of technical terms. Each chapter is introduced by a provocative discussion in a popular vein of the basic principles or unsolved problems of the subject matter in the chapter. The chapter on igneous geology is introduced by a three-page description of the volcanic activity at Paricutin. The history of geologic ideas is touched lightly but pertinently throughout the book.

The illustrations, taken from existing geologic examples, are mostly new; only the best of the illustrations used in previous editions have been retained. Numerous well-chosen photographs are lined and lettered to bring out geologic features better than subscript descriptions can, and some with explanatory line drawings clarify the subject matter.

Physiographic maps of major physical regions of Middle North America on the inside front cover and of the oceans and continents with the principal highlands of the world on the inside back cover, to which references are made in the text, were drawn by Erwin Raisz. The data are up-to-date, readable, and teachable and will be welcomed by both students and teachers of elementary geology.

JACOB FREEDMAN

Department of Geology
Franklin and Marshall College

Two Years in the Antarctic. E. W. Kevin Walton. Philosophical Library, New York, 1955. 194 pp. Illus. + plates. $4.75.

Two Years in the Antarctic is a book equally fascinating to those who have practical experience of polar travel and to those who are only able to adventure vicariously. Kevin Walton has managed to write a story which, by its truth and accuracy, brings a sense of nostalgia to those who have experienced such conditions. It describes the setting up of a base, the difficulties and the pleasures of antarctic travel, the affection a man acquires for his dogs, the improvisations and the techniques of travel. All this, and much more, makes it an interesting, lively, and human account.

The advent of an American expedition complicated the life and plans of the British party, but soon the early difficulties disappeared and joint journeys were undertaken. Thus the cooperation of American aircraft with British dog-teams made possible a major piece of exploration. One also reads of hazardous flying by American pilots who searched for and found the crew of a crashed British aircraft after they had struggled homeward for 9 days. On another occasion it was a British party that rescued an American skier who had fallen 120 feet into a crevasse.

All through the kaleidoscopic story one feels the author's enthusiasm for the life he was leading and, with him, one feels regret when it is all over. No one could read this book without gaining a real understanding of what living and traveling in the Antarctic means. Throughout the book the details and major events add up to a true story well told, a story that reveals the characters of the men, both British and American, and more particularly how they adjusted themselves to their environment and to each other.

The book is well illustrated and there are two maps.

V. E. FUCHS

The Trans-Antarctic Expedition, London, England

Science Reasoning and Understanding. A handbook for college teachers. Intercollege Committee on the Evaluation of Science Objectives of the Cooperative Study of Evaluation in General Education. Paul L. Dressel, director. Brown, Dubuque, Iowa, 1954. vii + 233 pp. Illus. Paper, $3.50.

Seventeen cooperating institutions were represented in the Cooperative Study of Evaluations in General Education, Natural Science Committee. This committee took as its aim the clarification and precise statement of "the important objectives of general education in science, and effective means of achieving them." Although a number of objectives were recognized as important, it was agreed that only three would be tested in attempts to discover whether courses in science for nonscience majors in colleges actually achieved the objectives. These were to develop students who will be able (i) to apply science knowledge to new problems and situations, (ii) to read and evaluate news articles and popular writings on scientific developments; (iii) to understand the point of view with which a scientist approaches his problems, and the kinds of things he does. A series of tests was constructed on the basis of popular scientific articles, which were reproduced in whole or in excerpt form, together with questions about them.

A considerable part of the book is devoted to a reproduction of portions of books and of magazine articles, with comments concerning the way in which they may be used to sharpen the students' abilities in science. Included are also sample questions that students may be asked in order to see whether they have or have not used the articles to develop the abilities in question.

This book will surely be of interest to all science teachers and may help them to sharpen their own sense of what they are doing. Yet I cannot help having the feeling that a little too much confidence is placed in a mechanical device, the analysis of popular science articles, as if therein lay a simple key to the disturbing and

difficult problems of modern science education. I must confess to a personal bias, but to me it seems as if a clever trick were being introduced in place of sound instruction in scientific principles and, perhaps, at the expense of making those scientific principles meaningful in terms of their historical and social background and their philosophic and practical implications.

An excellent feature of the book is a "bibliography with annotations of college science and general education, 1951–1953" by Vaden W. Miles.

I. Bernard Cohen
Department of the History of Science, Harvard University

The Chemistry of Living Cells. Helen R. Downes. Harper, New York, 1955. x + 549 pp. Illus. $7.50.

In about 540 pages the author presents a remarkably concise and at the same time highly readable survey of biochemistry. This textbook is intended as an introduction to the subject for students of chemistry and the biological sciences. Although the organization of the material is not after the fashion of medical biochemistry, neither is there much of comparative biochemistry, which would be of greatest value to students of biology. A feature of the book that makes it well worth recommending is the historical approach to many of the classical chemical problems discussed; there are also footnotes of a biographical nature related to the names of biochemists associated with important discoveries.

L. J. Mullins
Biophysical Laboratory, Purdue University

Botany Principles and Problems. Edmund W. Sinnott and Katherine S. Wilson. McGraw-Hill, New York, ed. 5, 1955. vii + 528 pp. Illus. $6.75.

Teachers who have used former editions of *Botany Principles and Problems* by E. W. Sinnott will be pleased with the fifth edition of this excellent textbook by Sinnott and his associate Katherine S. Wilson. Teachers who have never used the book will do well to give it careful consideration.

The authors have retained the clear, simple, and concise presentation that has characterized *Botany Principles and Problems* since the first edition came from the press in 1923. The plant is presented as a living, functioning organism, and botany is presented as a science against a background of biology in general. The excellent stimulating questions and problems following each chapter, together with the lucid style of the authors, should create in students an attitude of interest, curiosity, and critical thinking.

In rewriting the text a number of changes were made in order and content, chief among which is a new chapter, "Plants and life," which emphasizes the importance of botany in the field of biology and the life of man in general. The second chapter, "Plant science and its development," has been enlarged to include a new section on the development of science in general and the scientific attitude. The chapter on "Metabolism" has been completely rewritten and improved, and the more technical aspects of the treatment of photosynthesis have been removed from the chapter on "The leaf" and included in this chapter. Other minor shifts in basic material from one chapter to another and the rearrangement of some of the chapters are evident. The illustrations are much improved over those used in former editions, more than 230 new figures having been added and many old drawings redone. The photographs are of excellent quality and interest.

The change in format of the book resulting in larger pages printed in two columns increases the ease of reading. The publishers have done a better job with the fifth edition than with any of the previous editions.

Even though the sequence of the chapters may not be approved by many teachers of general botany, the book will continue to be a valuable work in the field of general botany textbooks.

Robert F. Smart
Department of Biology, University of Richmond

The Elements of Chromatography. Trevor Illtyd Williams. Philosophical Library, New York; Blackie, London, 1955. 90 pp. Illus. + plates. $3.75.

As the title indicates, this book describes only the essentials of chromatography. The eight chapters cover the following topics: historical introduction, adsorption chromatography, partition chromatography, ion-exchange chromatography, miscellaneous forms of chromatography, treatment of colorless substances, the process of development, and chromatography in industry. The small format does not allow complete coverage of these topics, except the first. This chapter is particularly outstanding because it gives a very thorough survey of the early history of chromatography.

The space allotted to the major topics is well balanced, with the discussion of the less familiar techniques, such as paper electrophoresis, reverse phase chromatography, and chemically modified paper, confined to the chapter on miscellaneous methods. There are conspicuously few references. This objection, however, is partially removed by the inclusion of a bibliography with 17 general references at the end of the book. Diagrams, tables, and color plates are well chosen to illustrate the applicability of the methods and to a certain extent their practicality.

The book is printed on good quality art paper; and while this contributes greatly to the quality of the typography and color-plate reproductions, it does produce some eyestrain from the contrast and surface glare. The volume is well bound and very readable. There are very few typographic errors and one incorrect reference to reverse phase chromatography in the index.

The teacher, research worker, or supervisor who desires a general survey of chromatography will find this work interesting as well as useful.

James D. O'Rourke
Department of Chemistry, University of Michigan

Soil Fertility. C. E. Millar. Wiley, New York; Chapman and Hall, London, 1955. xi + 436 pp. Illus. $6.75.

The late C. E. Millar was a recognized authority in the field of soil science. This book reflects his rich experience in teaching soils and his work in directing soil research at Michigan State University. It will be a welcome and useful book, not only for students majoring in soils and agronomy at the senior and graduate level, but also for others, including county agents, soil conservation technicians, and farm managers, who have had basic courses in chemistry and soils.

Highly technical fundamental principles governing the behavior of soils and plant nutrition are presented with such clarity that they can be understood readily by those not trained especially in soil science. The author gives especial emphasis to the fundamental principles of soil fertility and shows how these principles can be put to practical use in solving the many problems of soil management and plant growth.

The major plant nutrients and the more important micronutrients are discussed from the viewpoint of (i) supply in the soil, (ii) factors affecting availability to plants, (iii) functions in plant growth, and (iv) symptoms of deficiency. The effect of moisture, light, air, and temperature on the functioning of both the roots and tops of plants is discussed.

The use of fertilizers and the comparative value of different cropping systems are treated on a regional basis. Various methods for determining nutrient deficiencies by rapid soil tests, plant analyses, and tissue tests are compared. Included is the theory of the famous Mitscherlich method.

The book contains an up-to-date review of research findings from both the laboratory and field experimental plots. An extensive list of references adds value to a comprehensive and practical book.

ROY D. HOCKENSMITH
Soil Conservation Service U.S. Dept. of Agriculture

Minerals in World Industry. Walter H. Voskuil. McGraw-Hill, New York, 1955. vii + 324 pp. Illus. $5.75.

The technologic, economic, geographic, and political influences of the world's minerals are the subject of this textbook on mineral economics. Starting with iron, to which almost one-quarter of the book is devoted, and progressing through energy and fuel resources, basic and light metals, building materials, and mineral plant foods to minor minerals, the author describes major producing and resource areas, shows how each contributes to industrial productivity, and suggests the role that each plays in international relations. The descriptions of particular characteristics of the mining and processing of major minerals are the most valuable contribution.

In his interpretation of the influence of minerals on economic and political relations, however, Voskuil appears at times to be emulating the late Ellsworth Huntington. Whereas the latter saw climate as the all-important factor, Voskuil credits minerals with a similar deterministic role. He writes, for example, "The United States holds supremacy in steel production because we have abundant iron ore of good quality" (p. 35); "Europe's wealth lies in her minerals" (p. 53); "It is true that the mere presence of coking coal and iron ore determines where industry can be located" (p. 28).

One must also question the importance that Voskuil attaches to iron ore as a locational factor in the iron and steel industry. To agree that "the blast-furnace method is limited to those relatively few areas where the three raw materials (iron ore, coking coal, and limestone) are close at hand . . ." (p. 30) either requires a very broad interpretation of what is "close," or overlooks such long-distance shipments of iron ore as those from Chile, Venezuela, Liberia, and Sweden to the United States or from Magnitogorsk to the Kusnetsk Basin in the U.S.S.R.

Despite these criticisms, *Minerals in World Industry* provides well-organized and valuable accounts of the world's major minerals.

WILLIAM A. HANCE
Department of Economic Geography,
Columbia University

Experiments in Organic Chemistry. Louis F. Fieser. Heath, Boston, ed. 3, 1955. 359 pp. Illus. + plates. $5.25.

The two general philosophies of approach to the laboratory work in organic chemistry may be described as academic and research. The former approach presents a wide coverage of topics with emphasis on a close correlation between laboratory and lecture topics. The latter approach (exemplified by Fieser's book) emphasizes laboratory methods and a more detailed assignment of specialized topics. This research approach caters to the needs of the student who plans advanced work in chemistry.

The difference in external appearance of the third edition from the previous editions of Fieser's well-known manual prepares one for the changes within. Although the basic structure remains the same, the new edition contains many changes, extensive revisions, a number of deletions, and certain innovations. From the author's preface, "New experiments are presented on elution and paper-strip chromatography, enzymatic and spontaneous resolution, synthesis of ninhydrin, isolation of plant and animal products, synthesis of *cis-* and *trans-*stilbene, tolan, and related products (the infrared and ultraviolet spectra of which are reproduced). Another new chapter is an exercise in cost calculation."

The emphasis on laboratory technique is exemplified by a combination of semimacro and semimicro experimentation with newly designed types of apparatus. These pieces are available at moderate cost or may be produced from commonly available items. The users of this type of manual will welcome the changes in part I.

Part II (section on advanced work) was revised in

the interests of its users. The valuable sections on advanced techniques, methods, and solvents show careful revision and additions. The added chapter on reagents lists 320 general and special reagents with 870 references to the literature. The text includes valuable tables on the inside covers. An instructor's manual, available at nominal cost, enhances the teaching value of the laboratory textbook.

ROY G. BOSSERT

Department of Chemistry, Ohio Wesleyan University

The New Warfare. C. N. Barclay. Philosophical Library, New York, 1954. x + 65 pp. $2.75.

The well-known editor of the British journal, *The Army Quarterly*, analyzes that uncertain state of disequilibrium which we commonly call "cold war." The root cause of this condition is a cleavage between the ideologies of Soviet Communism and "free democracy." The new type of warfare resulting from this consists in hostile actions and provocations, which at other times in history would have led to open warfare. Obstructions, annoyances, provocations, and even occasional forays will continue for many years to come. We must understand that this is not a transient phenomenon, and we must develop new attitudes and techniques to cope with this kind of warfare.

Barclay has no spectacular solution to this, the paramount problem of our time. He suggests that the United States must acknowledge the right of others to live under their own form of government, however, repugnant that form may be to us. Second, the Soviet Union must agree not to use forceful means to convert the whole world. These two principles are restatements of the policies of coexistence and containment. Barclay proposes that the acceptance of these two principles be achieved through a series of high-level conferences among the major powers of the world. International tensions will, after a period of several years, thereby be reduced. This solution may appear disarmingly simple, but indeed it, or a variant of it, is the only alternative to catastrophic destruction.

RALPH BRAIBANTI

Department of Political Science, Duke University

Applied Entomology. An introductory textbook of insects in their relations to man. H. T. Fernald and Harold H. Shepard. McGraw-Hill, New York–London, ed. 5, 1955. ix + 385 pp. Illus. $7.

This book is one of the well-known series of McGraw-Hill Publications in the Agricultural Sciences issued under the consulting editorship of R. A. Brink. Originally published in 1921, this work is already known in its various revised editions of 1926, 1935, and 1942. It is therefore not necessary here to enlarge on its scope, since this follows the general arrangement of previous editions. However, much of the text has had to be rewritten owing to the fact that, since the fourth edition appeared, the development of the new organic insecticides has revolutionized insect control. The chemical control of a number of major pests has become practical for which previously no effective method was known. In this field greater advances have been made since World War II than in the previous quarter-century or more. Insecticides such as DDT, benzene hexachloride, and organic phosphorus compounds in large measures have replaced lead arsenate, calcium arsenate, lime-sulfur and nicotine sulfate.

There have been a number of noteworthy changes in this new fifth edition. Sections have been completely reorganized, and space formerly devoted to noneconomic species, such as certain large moths and butterflies, has been reduced. More emphasis is given to insect physiology, and species that have recently become major pests are introduced; likewise more space is now devoted to parasitic Hymenoptera as well as common ants and wasps. Many new illustrations from previously unpublished photographs are included; new information concerning the biology and distribution of various insects has been incorporated, and a section is devoted to consideration of other pest animals that are more or less closely related to insects. In the discussion of insect classification numerous examples are given of species conspicuous for being either very harmful or decidedly beneficial. On the whole, the very high standard of excellence of the previous editions is maintained, and the book will continue its wide usefulness.

J. S. WADE

Washington, D.C.

Physics: a Descriptive Interpretation. C. H. Bachman. Wiley, New York; Chapman & Hall, London, 1955. viii + 497 pp. Illus. $5.50.

This is a descriptive interpretation of physics, as the title claims, and not a worshipful compilation of historical names. For a person at all familiar with the phenomena that make up the universe that can be investigated by measurement, the author's discussion and illustration of things physical is fascinating. It would seem that almost anyone who reads should find the time spent in reading this book definitely instructive and entertaining. The fact that the book is designed for a textbook should not frighten away those who are interested or curious about what is going on around them these days. The book should prove to be good reading even for journalistic majors interested only in getting "credit" for a course in science.

For most physics teachers this textbook will supply a point of view and a wealth of illustration well worth the reading. Such a reader should have fun keeping track of the physics covered while ignoring such famous names as Newton, Galileo, and Boyle. The author does not quite play fair, however, because we do find Planck, Bohr, Ohm, and Pascal at least getting their names in print. The author does stick to physics, and in no sense can he be considered a booster for big names. The result is refreshing and vivid from the point of view of the physics involved.

In my opinion the author shows sound judgment and facility in the sequence of subjects covered. The plot develops rapidly with an early discussion of all the atomic and nuclear particles that are getting publicity these days. The nature of the description of the measurables, although it is largely qualitative, is clear and accurate in more cases than would be guessed possible without resort to more quantitative definitions. However, the descriptions of some basic concepts could be easily improved. For example, the author does not distinguish well between electromagnetic waves and the energy transferred by such waves. There is also a touch of carelessness indicated in such italicized statement as "Temperature is an indication of the heat the body possesses."

As a possible textbook to be used in such a course as that for which it was developed, there is much to recommend it. Some culturists will object to this book's lack of the historical, but for those who feel that some understanding of the fundamentals of the physical universe should be a part of the well-educated and "adjusted" college graduate, this textbook should be a "best seller." In these days of diesel engines, television, jet propellers, heat pumps, going through McGuffey's readers no longer constitutes an education. The old gentleman who 75 years ago remarked that he wanted his grandchildren to be educated clear through the "fourth reader" so "they could travel all over the world and not get cheated" would now demand a college education for his boys and probably a knowledge of as much physics as that covered in Bachman's textbook. They would not be so easily cheated.

R. F. PATON
Department of Physics, University of Illinois

Books Reviewed in SCIENCE

5 August

Fluorine Chemistry, J. H. Simons, Ed. (Academic). Reviewed by E. T. McBee.

An Introduction to Plant Taxonomy, G. H. M. Lawrence (Macmillan). Reviewed by L. Constance.

The Gyroscope Applied, K. I. T. Richardson (Philosophical Library). Reviewed by F. W. Sears.

Fundamentals of Radiobiology, Z. M. Bacq and P. Alexander (Academic). Reviewed by R. F. Kimball.

The Nucleic Acids, E. Chargaff and J. N. Davidson, Eds. (Academic). Reviewed by R. D. Hotchkiss.

Radioisotope Conference, 1954, vol. I, J. E. Johnston, Ed. (Academic; Butterworths). Reviewed by E. L. Bennett.

The Plant Quarantine Problem, W. A. McCubbin (Munksgaard; Chronica Botanica.) Reviewed by E. P. Reagan.

Glass Reinforced Plastics, P. Morgan, Ed. (Iliffe; Philosophical Library). Reviewed by C. L. Babcock.

Peripheral Nerve Injuries, H. J. Seddon, Ed. (Her Majesty's Stationery Office). Reviewed by E. Alexander, Jr.

12 August

Analysis of Development, B. H. Willier, P. A. Weiss, V. Hamburger, Eds. (Saunders). Reviewed by J. Needham.

University Physics, F. W. Sears and M. W. Zemansky (Addison-Wesley). Reviewed by R. M. Sutton.

Pathology of the Dog and Cat, F. Bloom (American Veterinary Publications). Reviewed by J. T. McGrath.

Higher Transcendental Functions, A. Erdelyi, Ed. (McGraw-Hill). Reviewed by R. P. Boas, Jr.

World Outside My Door, O. B. Goin (Macmillan). Reviewed by P. F. Brandwein.

Biochemistry of Nitrogen, N. J. Toivonen *et al.* (Suomalainen Tiedeakatemia). Reviewed by R. C. Corley.

Radioisotope Conference, 1954, vol. II, J. E. Johnston, Ed. (Academic; Butterworths). Reviewed by R. M. Lemmon.

Outlines of Enzyme Chemistry, J. B. Neilands and P. K. Stumpf (Wiley; Chapman & Hall). Reiewed by E. Racker.

The Theory of Cohesion, M. A. Jaswon (Interscience; Pergamon). Reviewed by F. H. Herbstein.

Substances Naturelles de Synthèse, L. Velluz, Ed. (Masson). Reviewed by A. G. Long.

Variational Principles in Dynamics and Quantum Theory, W. Yourgrau and S. Mandelstam (Pitman). Reviewed by P. G. Bergmann.

19 August

Advances in Protein Chemistry, vol. IX, M. L. Anson, K. Bailey, J. T. Edsall, Eds. (Academic). Reviewed by R. C. Warner.

Marine Shells of the Western Coast of Florida, L. M. Perry and J. S. Schwengel (Paleontological Research Institution). Reviewed by L. J. and M. Milne.

Applied X-rays, G. L. Clark (McGraw-Hill). Reviewed by R. Pepinsky.

Vorlesungen über Differential- und Intergralrechnung, vols. 1, 2, 3, A. Ostrowski (Birkhauser). Reviewed by W. Kaplan.

Acetylenic Compounds in Organic Synthesis, R. A. Raphael (Academic; Butterworths). Reviewed by T. L. Jacobs.

Lectures on Partial Differential Equations, I. G. Petrovsky (Interscience). Reviewed by L. Bers.

A Symposium on Amino Acid Metabolism, W. D. McElroy and H. B. Glass, Eds. (Johns Hopkins Press). Reviewed by R. R. Williams.

Abstract Bibliography of Cotton Breeding and Genetics, 1900–1950, R. L. Knight (Commonwealth Agricultural Bureaux). Reviewed by T. Kerr.

Quantitative Methods in Histology and Microscopic Histochemistry, O. Eranko (Karger; Little, Brown). Reviewed by R. D. Lillie.

Variable Stars and Galactic Structure, C. Payne-Gaposchkin, (Athlone; de Graff). Reviewed by O. Struve.

Modern Gas Analysis, P. W. Mullen (Interscience). Reviewed by P. Fugassi.

An Introduction to the Study of Insects, D. J. Borrer and D. M. DeLong (Rinehart). Reviewed by R. W. Sherman.

An Annotated Bibliography of Submarine Technical Literature: 1557 to 1953, Committee on Undersea Warfare (National Research Council).

26 August 1955

The Dancing Bees, K. von Frisch (Harcourt Brace). Reviewed by C. D. Michener.

Elements of Servomechanism Theory, G. J. Thaler (McGraw-Hill). Reviewed by C. W. Miller.

Biology of Deserts, J. W. Cloudsley-Thompson, Ed. (Inst. of Biology, London). Reviewed by E. K. Reed.

The Skin, a Clinicopathologic Treatise, A. C. Allen (Mosby). Reviewed by Walter Teichmann.

Solubilization and Related Phenomena, M. E. L. McBain and E. Hutchinson (Academic). Reviewed by I. M. Kolthoff.

Bibliography on Hearing, S. S. Stevens, J. G. C. Loring, D. Cohen, compilers (Harvard Univ. Press). Reviewed by E. G. Wever.

Integers and Theory of Numbers, A. A. Fraenkel (Scripta Mathematica). Reviewed by J. Dyer-Bennet.

The Nitrogen Metabolism of Micro-organisms, B. A. Fry (Wiley). Reviewed by S. G. Knight.

Biochemistry and Physiology of Protozoa, vol. II, S. H. Hutner and A. Lwoff, Eds. (Academic). Reviewed by R. R. Kudo.

Ciba Foundation Symposium on Chemistry and Biology of Pteridines, G. E. W. Wolstenholme and M. Cameron, Eds. (Little, Brown). Reviewed by E. L. R. Stokstad.

Chemisorption, B. M. W. Trapnell (Academic; Butterworths). Reviewed by G. E. Boyd.

New Books

Perinatal Mortality in New York City: Responsible Factors. A study of 955 deaths by the Subcommittee on Neonatal Mortality, Committee on Public Health Relations, New York Acad. of Medicine. Schuyler G. Kohl. Harvard Univ. Press, Cambridge, 1955. 111 pp. $2.50.

College Algebra and Plane Trigonometry. Abraham Spitzbart and Ross H. Bardell. Addison-Wesley, Cambridge, 1955. 408 pp. $4.50.

The Nucleic Acids: Chemistry and Biology. vol. II. Erwin Chargaff and J. N. Davidson, Eds. Academic Press, New York, 1955. 576 pp. $14.50.

Measurement and Evaluation in Psychology and Education. Robert L. Thorndike and Elizabeth Hagen. Wiley, New York; Chapman & Hall, London, 1955. 575 pp. $5.50.

Essentials of Biological and Medical Physics. Ralph W. Stacy, David T. Williams, Ralph E. Worden, and Rex O. McMorris. McGraw-Hill, New York–London, 1955. 586 pp. $8.50.

Everything and the Kitchen Sink. How the first century of industry created our first century of good living. Farrar, Straus & Cudahy, New York, 1955. 160 pp. $4.

Highway to the North. Frank Illingworth. Philosophical Library, New York, 1955. 293 pp. $7.50.

Quantum Theory of Solids. International Ser. of Monogr. on Physics. R. E. Peierls. Oxford Univ. Press, New York–London, 1955. 229 pp. $4.80.

Dielectric Behavior and Structure. Dielectric constant and loss, dipole moment and molecular structure. Charles Phelps Smyth. McGraw-Hill, New York–London, 1955. 441 pp. $9.

Materials for Nuclear Power Reactors. Pilot Books, No. 7. Henry H. Hausner and Stanley B. Roboff. Reinhold, New York, 1955. 224 pp. $3.50.

Principles of Mathematics. C. B. Allendoerfer and C. O. Oakley. McGraw-Hill, New York–London, 1955. 448 pp. $5.

Estudos Sobre os Virus Coxsackie. Paulo de Goes. Universidade do Brasil, Rio de Janeiro, 1954. 271 pp.

Laboratory Studies in Biology: Observations and Their Implications. Chester Lawson, Ralph Lewis, Mary Alice Burmester, and Garrett Hardin. Freeman, San Francisco, 1955. 328 pp. $3.50.

Horticultural Science. A reading and laboratory manual. Gordon T. Nightingale. Horticultural Publications, Rutgers Univ., New Brunswick, N.J., 1955. 111 pp.

The Underwater Naturalist. Pierre de Latil. Trans. by Edward Fitzgerald. Houghton, Mifflin, Boston, 1955. 275 pp. $3.50.

Radioisotopes in Biology and Agriculture: Principles and Practice. C. L. Comar. McGraw-Hill, New York–London, 1955. 481 pp. $9.

Petrographic Mineralogy. Ernest E. Wahlstrom. Wiley, New York; Chapman & Hall, London, 1955. 408 pp. $7.75.

Symposium on Atherosclerosis. Publ. 338. National Acad. of Sciences–National Research Council, Washington, 1954. 249 pp. $2.

The Biologic Effects of Tobacco. With emphasis on the clinical and experimental aspects. Ernest L. Wynder, Ed. Little, Brown, Boston, 1955. 215 pp. $4.50.

Tea. A symposium on the pharmacology and the physiologic and psychologic effects of tea. Henry J. Klaunberg, Ed. Biological Sciences Foundation, Washington, D.C., 1955. 64 pp. Paper, $1.

College Physiology. Donald M. Pace and Benjamin W. McCashland. Crowell, New York, 1955. 615 pp. $5.50.

Anxiety and Stress. An interdisciplinary study of a life situation. Harold Basowitz, Harold Persky, Sheldon J. Korchin, and Roy R. Grinker. Blakiston Div., McGraw-Hill, New York–London, 1955. 320 pp. $8.

Studies of Biosynthesis in Escherichia Coli. Publ. 607. R. R. Roberts, P. H. Abelson, D. B. Cowie, E. T. Bolton, and R. J. Britten. Carnegie Institution of Washington, Washington, 1955. 521 pp. Paper, $2.50; cloth, $3.

Embryogenesis in Plants. C. W. Wardlaw. Wiley, New York; Methuen, London, 1955. 381 pp. $7.

Some Physiological Aspects and Consequences of Parasitism. William H. Cole, Ed. Rutgers Univ. Press, New Brunswick, N.J., 1955. 90 pp. Paper, $2.

The Hunting Wasp. John Crompton. Houghton Mifflin, Boston, 1955. 240 pp. $3.

Quantitative Analysis: Methods of Separation and Measurement. M. G. Mellon. Crowell, New York, 1955. 694 pp. $6.50.

Gas Dynamics of Cosmic Clouds. A symposium of the International Union of Theoretical and Applied Mechanics and International Astronomical Union. Interscience, New York; North-Holland, Amsterdam, 1955. 247 pp. $5.75.

The Story of FAO. Gove Hambidge. Van Nostrand, New York–London, 1955. 303 pp. $6.50.

General Principles of Geology. J. F. Kirkaldy. Philosophical Library, New York, 1955. 327 pp. $6.

Walt Whitman's Concept of the American Common Man. Leadie M. Clark. Philosophical Library, New York, 1955. 178 pp. $3.75.

Breeding Beef Cattle for Unfavorable Environments. A symposium presented at the King Ranch Centennial Conference. Albert O. Rhoad, Ed. Univ. of Texas Press, Austin, 1955. 248 pp. $4.75.

Geology: Principles and Processes. William H. Emmons, George A. Thiel, Clinton R. Stauffer, and Ira S. Allison. McGraw-Hill, New York–London, ed. 4, 1955. 638 pp. $6.50.

Advanced Calculus. Angus E. Taylor. Ginn, Boston, 1955. 786 pp. $8.50.

Practical Laboratory Chemistry. A manual for beginners. Horace G. Deming. Wiley, New York; Chapman & Hall, London, 1955. 209 pp. $3.50.

Culture and Mental Disorders. Joseph W. Eaton. Free Press, Glencoe, Ill., 1955. 254 pp. $4.

Differential Equations. Frederick H. Steen. Ginn, Boston, 1955. 330 pp. $4.

Security for All and Free Enterprise. A summary of the social philosophy of Josef Popper-Lynkeus. Henry I. Wachtel, Ed. Philosophical Library, New York, 1955. 162 pp. $3.

Treatise on Invertebrate Paleontology. part E, *Archaeocyatha and Porifera.* Prepared under the guidance of the Joint Committee on Invertebrate Paleontology. Raymond C. Moore, Ed. Univ. of Kansas Press, Lawrence; Geological Soc. of America, New York 27, 1955. 122 pp. $3.

Danger My Ally. F. A. Mitchell-Hedges. Little, Brown, Boston, 1955. 278 pp. $3.75.

Blutgerinnungsfaktoren. Band I. Erwin Deutsch. Franz Deuticke, Wien, 1955. x + 298 pp. Illus. DM. 42.

Merchant Ships: World Built. 1955 volume. De Graff, New York, 1955. 264 pp. $5.

European Firearms. J. F. Hayward. Philosophical Library, New York, 1955. 53 pp. $7.50.

Radiocarbon Dating. Willard F. Libby. Univ. of Chicago Press, Chicago, ed. 2, 1955. 175 pp. $4.50.

Problems of Consciousness. Transactions of the fifth conference. Harold A. Abramson, Ed. Josiah Macy, Jr. Foundation, New York, 1955. 180 pp. $3.50.

The Natural History of Tsetse Flies. An account of the biology of the genus *Glossina* (Diptera). Memoir No. 10. London School of Hygiene and Tropical Medicine. Lewis, London, 1955. 816 pp. £4 4s.

Basic Processes of Gaseous Electronics. Leonard B. Loeb. Univ. of California Press, Berkeley, 1955. 1012 pp. $13.50.

High Energy Nuclear Physics. Proceedings of the fifth annual Rochester conference, 31 Jan.–2 Feb. 1955. Compiled and edited by H. P. Moyes, E. M. Hafner, G. Yekutieli, and B. J. Raz. Univ. of Rochester, Rochester, N.Y.; Interscience, New York, 1955. 198 pp. $2.50.

A Laboratory Manual of General Chemistry. Saverio Zuffanti, Arthur A. Vernon, and W. F. Luder, Saunders, Philadelphia–London, 1955. 308 pp. $3.75.

Physiologie der Zelle. Johannes Haas. Borntraeger, Berlin, 1955. 474 pp. DM. 48.

Adaptive Human Fertility. Paul S. Henshaw. Blakiston Div., McGraw-Hill, New York–London, 1955. 322 pp. $5.50.

Salamanders and Other Wonders. Still more adventures of a romantic naturalist. Willy Ley. Viking, New York, 1955. 293 pp. $3.95.

The Story of Medicine. Kenneth Walker. Oxford Univ. Press, New York, 1955. 343 pp. $6.

Standing Room Only. The challenge of overpopulation. Karl Sax. Beacon Press, Boston, 1955. 206 pp. $3.

Cancer through the Ages the Evolution of Hope. Francelia Butler. Virginia Press, Fairfax, 1955. 147 pp. Illus. Paper, $1.

Experimental Design and Its Statistical Basis. D. J. Finney. Univ. of Chicago Press, Chicago, 1955. 169 pp. $4.50.

The Physiology of Domestic Animals. H. H. Dukes. Comstock Publishing Associates Div., Cornell Univ. Press, Ithaca, N.Y. ed. 7, 1955. 1020 pp. $9.75.

Vascular Plants of Illinois. George Neville Jones, George Damon Fuller *et al.* Univ. of Illinois Press, Urbana, 1955. 593 pp. $10.

College Chemistry. An introductory textbook of general chemistry. Linus Pauling. Freeman, San Francisco, ed. 2, 1955. 685 pp. $6.

Report Writing. John Ball and Cecil B. Williams. Ronald Press, New York, 1955. 407 pp. $4.75.

Practical Physics. Marsh W. White, Kenneth V. Manning, and Robert L. Weber. McGraw-Hill, New York–London, ed. 2, 1955. 484 pp. $5.50.

Mesons and Fields. vol. II, *Mesons.* Hans A. Bethe and Frederic de Hoffmann. Row, Peterson, Evanston, Ill., 1955. 446 pp. $8.

Histologische Geschwulstdiagnostik. Systematische morphologie der menschlichen geschwulste als grundlage fur die klinische beurteilung. A. V. Albertini. Thieme, Stuttgart, 1955. 544 pp. $23.40.

Blood Supply and Anatomy of the Upper Abdominal Organs. With a descriptive atlas. Nicholas A. Michels. Lippincott, Philadelphia, 1955. 581 pp. Illus. $24.

Introduction to Parasitology. With special reference to the parasites of man. Asa C. Chandler. Wiley, New York; Chapman & Hall, London, ed. 9, 1955. 799 pp. $8.50.

Corn and Corn Improvement. Agronomy Monogr. vol. 5. George F. Sprague. Ed. Academic Press, New York, 1955. 699 pp. $11.50.

New Methods in Analytical Chemistry. Ronald Belcher and Cecil L. Wilson. Reinhold, New York; Chapman & Hall, London, 1955. 287 pp. $5.50.

Some Beautiful Indian Trees. Ethelbert Blatter and Walter Samuel Millard. The Bombay Natural History Soc., Bombay, India, ed. 3, 1955. 165 pp. 30s.

Laboratory Identification of Pathogenic Fungi Simplified. Elizabeth L. Hazen and Frank Curtis Brown. Thomas, Springfield, Ill. 1955. 108 pp. $5.50.

Society and Medicine. Lectures to the Laity, No. XVII. Iago Galdston, Ed. International Universities Press, New York, 1955. 131 pp. $3.

Geologische Bau der Sudamerikanischen Kordillere. Heinrich Gerth. Gebruder Borntraeger, Berlin, 1955. 264 pp. DM. 52.50.

Harmonic Analysis and the Theory of Probability. Salomon Bochner. Univ. of California, Berkeley, 1955. 176 pp. $4.50.

Scientific and Technical Societies of the United States and Canada. Natl. Acad. of Sciences–Natl. Research Council, Washington, ed. 6, 1955. 441 pp. $7.50.

Grundlagen der Analytischen Chemie und der Chemie in Wässrigen Systemen. Fritz Seel. Verlag Chemie, GMBH Weinheim/Bergstrasse, Germany, 1955. 348 pp. Illus. DM 29.

LETTERS

Usefulness of Test Items That Involve Finding a Pattern in Data: A Reply

In his recent article "Validity of test items that involve finding a pattern in data" [*Sci. Monthly* **80,** 50 (1955)], Richard H. Lampkin criticized on rational grounds the use of a certain type of item often found in tests used for the guidance and selection of scientific personnel. Although his criticisms are directed specifically at number series items, Lampkin urges his reader to extend the criticism to a great variety of items that ask the subject to formulate some general law and apply it to the data. He also suggests that the "creative act" in scientific research is unpredictable and generally leaves the impression that any attempt to make predictions about the success of persons entering scientific fields is doomed to failure.

Because the identification and placement of qualified individuals in scientific areas is of such great importance to our national welfare in particular and to society in general, I feel compelled to answer some of the questions that Lampkin has raised and to present some empirical evidence that it is possible, by means of tests, to pick out from a group of individuals those who are most likely to be successful in scientific pursuits.

In making this reply to Lampkin, I am not recommending that number series items as such be used in place of others. Rather, I hope (i) to describe the kinds of evidence on which the value of any item should be judged; (ii) to show that, in spite of some obvious imperfections, number series and other such items *are* useful in selection and guidance; and (iii) to point out that decisions with regard to future performance are wrong far more often when they are made without the use of tests than when they are made with them.

Before concerning ourselves with Lampkin's specific remarks, however, it is necessary to examine the reasoning behind the construction of mental tests.

Although the term *measurement* is often applied to the operation of giving a test, it should be understood that in this context it is not used in the everyday sense of the word. The mental tester does not have a set of items that form a standard yardstick having a zero point and equal units of measure, against which a human being can be compared to determine his capacity in some given area. Rather, a mental test represents some observations on human behavior in a well-controlled situation; and predictions made from mental tests are inferences based on empirical observations of human behavior. Specifically, the mental tester notes that most people who behave in a given way in his well-controlled situation behave in some particular way in a second situation, and that this behavior in the second situation is different from that of most of those who reacted in some other way to the original situation. Having made a large number of such observations on some representative or normative group of individuals, the tester is then able to predict how some new individual will behave in the second situation by making observations of his behavior in the controlled situation.

The kind of observations made in the controlled situation are often responses to a series of questions that are quantified by some scoring operation. As a result of this operation, each individual is assigned a number that is considered to be representative of his position on what is called a predictor. Similarly, the observations in the second situation are quantified, and the resulting set of numbers is said to represent the positions of the observed individuals on a criterion.

The kind of predictions made, as well as their accuracy, depends upon the relationship between the predictor and the criterion, as measured in the normative group. The over-all estimate of the accuracy of prediction is what is meant by the validity of the predictor, and this would be obtained by applying that particular statistical technique for estimating the degree of relationship between two variables that is appropriate for the particular kind of quantification of observations involved.

A simplified example of a set of data from which predictions of this sort are made is presented in Table 1 (*1*). These data represent some observations actually made on 361 students in a well-known institution of higher learning. The predictor in this instance is the score received on a test of mathematical reasoning that was taken before admittance to college, and the criterion is the average of the grades received in mathematics courses during the year. Here, for example, an individual who has behaved in such a way as to receive a value of 15 on the predictor would be said to have 7 chances in 100 of getting an average grade of A, 22 chances in 100 of getting an average grade of B, and so on.

There are two things about this procedure that are often neglected, and therefore special note should be taken of them. The first is that these predictions are statistical in nature. To say that any particular new individual who gets some specific score must succeed

Table 1. Estimated probability that a person in a given score interval will receive a given average course grade.

Predictor (mathematics test scores)	Number of persons per score interval	Criterion* (average grade in mathematics)				
		F	D	C	B	A
17–20	49		.08	.27	.51	.14
13–16	133	.01	.26	.44	.22	.07
9–12	97	.03	.42	.46	.08	.01
5– 8	65	.08	.58	.28	.06	
1– 4	17	.18	.53	.23	.06	

* For illustrative purposes the criterion was converted from the numerical score originally reported to letter grades.

or must fail is nonsense. Not only are the probabilities in any such expectancy table estimates made from a particular (and, unfortunately, often mighty small) sample of individuals, but also, since there are practical limitations to the number and kind of observations that can be made, the true probabilities are not likely to be a certainty. Nevertheless, *when practical situations demand that a prediction be made for a given individual, the estimated probabilities in an expectancy table provide the best evidence available.*

The second point of which special note should be made is that the validity of the predictor is based on empirical evidence of the relationship between the predictor and the criterion and not on any "correctness" or "truthfulness" in the particular scheme (for example, the scoring system in the case of a test predictor) used for quantifying the observations. A great deal of misunderstanding about the general nature of mental tests has arisen through failure to note that the term validity has this rather specialized meaning in psychometric prediction situations.

One final statement that should be made before turning to Lampkin's specific comments is that the predictor need not always be a test score, nor need the criterion always be achievement in a learning situation. Actually, with respect to the predictor, the aspects of behavior that are selected for observation and the way in which such observations are made and recorded are, within the limits of practical demands, arbitrary with the person making the predictions. He may use test scores, biographical data, information obtained in an interview, recommendations of others, or anything else that in some way reflects an individual's behavior. The extent to which useful predictions can be made, however, will depend upon the appropriateness of the behavior selected and the skill with which the observations were made and quantified. With respect to the criterion, however, the reasons for making the predictions will dictate which aspects of behavior should be considered. For example, if it is desired to select individuals who will be successful in scientific work, the criterion will be determined once what is meant by success can be specified. Even with these restrictions, however, the criterion can be quite varied; success in science might be judged by such different things as grades in science courses, highest degree an individual has received, salary in scientific work, judgment of colleagues, number of papers published, number of outstanding discoveries made, or any combination of such variables.

Although this outline of the logic behind the use of mental tests has been brief, and a great many important facets of the problems involved have barely been touched upon or neglected entirely, it will provide some background for the following discussion of Lamkin's paper.

The first criticism that Lampkin makes of test items that involve finding a pattern in data in general, and number series items in particular, is that more than one solution is always possible. Few, if any, test constructors would deny the fact so amply demonstrated by Lampkin that infinities of schemes are possible which would account for any series of numbers. Nor would any scientist deny that there are an infinite number of explanations for any particular set of data that he has collected. To get the correct solution in the case of a number series item such as that which Lampkin used for an illustration, one needs only to make use of two rules that most scientists who were not able to gather further data at the moment would follow: (i) accept the explanation that can be formulated on part of the data and tested on the rest of the data in preference to one that requires extrapolation to data not at hand, and (ii) accept the solution that is most simple. None of Lampkin's schemes can be formulated without postulating additional data, and there would be little disagreement that the rule "add two" is less complicated than any of the alternate solutions proposed by Lampkin.

Even more important than this, however, is the fact that the lack of plural solutions in such items is a desirable, rather than an essential, characteristic. Although the relative uniqueness of the possible responses to the item may provide a more adequate scoring system, the crucial judgment regarding the value of the item should be based on its predictive validity—that is, in terms of whether or not it will differentiate those who are later most successful from those who later are less successful.

An example of the kind of data on which such a judgment can be made is given in Table 2. These data were obtained on an item that was included in a test currently being constructed for use as a measure of scientific reasoning and understanding. The item describes a series of chemical experiments and asks which of several conclusions is justified from the data, and the criterion used was a rating, by teachers, of the scientific ability of the subjects involved. To the extent that persons who have high ability (according to the criterion) pass the item and those who have low ability fail the item, the item is useful.

A second objection raised by Lampkin to items of this general sort is that routine procedures will usually yield a solution. This is perhaps, his most serious criticism. However, in some instances it may even be desirable to have a training component in a test. This latter occurs if the person who knows and can apply specific techniques is more likely to produce results than is a person who does not know of, or is not able to apply, these methods.

When it is desirable to exclude special background and training, there are methods by which a test constructor can estimate the extent to which they are im-

Table 2. Proportion of high and low groups that passed or failed the item.

		Criterion	
		Low	High
Item	Pass	.55	.80
	Fail	.45	.20

portant in answering any item and thus use this information in selecting items that will be included in the final form of the test.

When Lampkin says "A person may have demonstrated repeatedly considerable ability to find pattern in data and still, in any particular problem situation, may fail while another with lesser reputation succeeds," he seems to be pointing out that, in making predictions about any particular person, we may be wrong. Because of the statistical nature of our predictions, this will always be so. The greater the amount of information available for making a prediction (that is, the larger the number of behavior situations in which we have been able to make observations and the larger the number of people we have been able to observe in these situations), the less the likelihood of this sort of error; but, having made maximum use of all available information, it will be impossible to tell whether John Jones will be the exception to the rule. The only consolation that the person making the prediction has is that, had he followed any other procedure, he would have made a larger number of errors; in place of just Jones, he would find Jim Smith, Bill Baker, and Joe Doakes to be the exceptions.

Whether or not, for any given situation, best use of all possible information is made by gathering the data through controlled or "field" observation, by combining the data statistically or subjectively, and by basing the predictions on empirical or intuitive probabilities is a question that has an empirical answer. Thus far, in situation after situation, the evidence has favored controlled observation, statistical combination, and empirical probabilities. [Readers interested in an important study, which is particularly appropriate to this discussion because it deals with high-level professional persons, should consult Kelly and Fiske(*2*).]

Finally, in making the statement that the finding of patterns in data "seems as unpredictable, as little responsive to demand, as is the creation of a masterpiece in painting or symphonic music," Lampkin gives the impression that, in the scientific area at least, both the use of present tests and the search for better predictors in the future should be abandoned. To place the accurate prediction of success in the creative fields with the impossible is, however, not to solve the problem of making the most efficient use of our human resources or to prevent the personal tragedy of a person who builds up his hopes and spends several years of his life struggling to achieve in an area where there is little hope of his success.

One of the recent reports of the National Manpower Council (*3*) indicates that approximately $100 million will be spent annually in the form of scholarships, stipends, and loans to encourage advanced study. Decisions must be made as to which individuals these grants and loans should be given. Not only is there a financial loss every time such a grant is given to a person who is not successful, but a young man who otherwise might be a happy and useful member of society finds himself a failure. Granted, the loss of even one highly productive individual through a wrong decision on the basis of a test score would be a serious one. It is apparent, however, that an even greater loss would occur had the decision been made on some other basis!

In conclusion, it would seem that, since predictions regarding the success of individuals in scientific pursuits have such a great national and personal utility, the best-known methods of making such predictions should be used. Up to the present time, the evidence has indicated that mental tests have provided the most accurate means by which such predictions can be made. In the construction of such tests, it would similarly seem far more appropriate to base the decision of whether or not an item should be included more on its demonstrated ability to discriminate between successful and unsuccessful persons than on the extent to which it conforms to some conceptual notion of what an item should be like. Therefore, until and unless it can be empirically demonstrated that items of a different sort can do a better job of discrimination, it is recommended that the use of items that involve finding patterns in data should be continued.

GERALD C. HELMSTADTER
Educational Testing Service, Princeton, New Jersey

References and Notes

1. The data of tables 1 and 2 were made available to me by John W. French and Gordon Loos, of the Educational Testing Service, Princeton, N.J.
2. E. L. Kelly and D. W. Fiske. "The prediction of success in the V. A. Training Program in Clinical Psychology," *Am. Psychol.* **5**, 395 (1950).
3. National Manpower Council, *A Policy for Scientific and Professional Manpower* (Columbia Univ. Press, New York, 1953).

Painting, Music, and Sunshine

One of the well-known contrasts between music and painting is that the former is almost entirely nonobjective, whereas the latter is basically imitative of nature. The most programmatic music makes but a very inferior medium for the communication of facts about the physical universe. On the other hand, painting traditionally has, at least as a point of departure, the simulation of our sensory impressions of nature—an imitation that ranges from sheer deception of the eye through varying degrees of abstraction in which form, line, and color suggest, with greater or less distortion, the world about us. It is safe to say that most of the world's great painting has been representational, in that it has depicted recognizable aspects of the material universe—abstracted in varying degrees—at least as starting points for the feelings and associations that the artists wished to convey. Truly nonobjective painting—that is, painting comparable with music in its lack of resemblance to the physical objects about us—is an exception to the tradition and enjoys a relatively limited popularity. Even the most popular music is nonrepresentational; "calendar art" never is. How has it come about that the great majority of people demand realism in painting but not in music?

The contrast is so marked that it could not fail to

attract considerable attention. The purpose of this note, however, is not to summarize the various explanations that have been advanced but rather to train some light on the problem from a perhaps unexpected source, namely, a comparison of the energy received by the eye with that received by the ear. The sun provides an uninterrupted stream of energy, the reflections of which bombard the eye with a continuous series of messages from objects about us. It is the *continuity* of reflected radiation, together with the variety of ways in which it is characteristically altered by the reflecting bodies, that makes sunlight the most useful form of energy available to us for the direct sensory perception of our physical surroundings. The mechanism is so superior to other means of orientation that in our evolution we have become largely visual animals, and the development of an elaborate system of communication by sounds (spoken words) has not lessened our primary dependence on sight for firsthand information concerning our surroundings.

Sounds, on the other hand, are of relatively infrequent and irregular occurrence. The principal ones (apart from the sounds of human voices and machines) are those made by wind and by water in motion. The addition of the noises made by other animals, thunder, landslides, avalanches, melting ice, and volcanoes practically completes the list, and none of these provides a continuous stream of energy whose reflections an animal, such as man, could use to apprise itself of the environment.

Because visual echoes (rather than sonic echoes) are the chief source of firsthand information concerning our physical environment, subjective reality is identified with, or at least strongly associated with, visual sensation. When we try to grasp difficult thoughts, we try to "picture" them. Therefore, abstractions are frequently most easily communicated when they are presented in visual form (such as graphs); we say that we cannot "see" so-and-so's theory, and that "seeing is believing." Because they have become so nearly identified with reality, visual sensations usually evoke at least subconscious associations with the material world about us. Indeed, the visual sense has become the slave of that part of the mind that is concerned with the interpretation of the physical universe. It is therefore inevitable that the mind should come to expect of the eye messages concerning the real world, and that it should tend to reject as less satisfying visual sensations that do not represent physical reality. Sensations of the latter type are produced by nonobjective painting, and for most people a considerable reeducation of the mind's eye is necessary before they can be appreciated. Thus, the eye's responsibility to inform the mind of our material surroundings has tended to restrict the scope of the visual arts.

Because sound is discontinuous and, hence, far less useful than light, the auditory sense has not developed a similar responsibility. Sounds are relatively "meaningless" (even language is abstract, that is, nonmimetic), and the ear is free to enjoy them for their own sakes. This is the case in music, a nonobjective art. One can speculate, however, that if the sun were a steady source of sound rather than of light, animals might have greatly developed their powers of hearing, and we would have learned to interpret the material world in terms of reflected sound. Suppose that light occurred only as irregular and disconnected flashes; then our sense of sight would probably have remained quite inferior. In this hypothetical universe, with hearing as the most informative of the senses and sound as the touchstone of reality, it is reasonable to assume that music would have been representational and the visual arts nonobjective.

DAVID M. PRATT

Narragansett Marine Laboratory,
University of Rhode Island, Kingston

One World—Or Two?

Is nature all one world, or two? In an article on "Parapsychology and dualism" [*Sci. Monthly* **79**, 1 (1954)] Roland Walker reaffirms a faith in mechanism as adequate to deal with all the phenomena of nature and berates the concept of dualism (or any modification of it under the guise of an ESP hypothesis) as leading to a smug self-satisfaction with having "explained" these phenomena by labeling them. This labeling, he holds, interferes with the scientifically desirable effort to find the true causes.

A great deal of the difference between the position of those who think as Walker does and that of most of the parapsychologists is the result of semantic difficulties—a basic misunderstanding of what is intended by the parapsychological concepts. The fundamental difference seems to hinge on Walker's definition of the term *mechanism*. According to Walker, "The idea of mechanism is here meant to include the full range of integrated causality." By Walker's interpretation, mechanism would have to include any and all cause-effect relationships in either a monistic or a dualistic concept of nature; mechanism thus becomes contrasted with animism or mysticism, and not with what the parapsychologists, as led by J. B. Rhine, mean by "psi" or nonphysical phenomena. This use of the term *mechanism* implies that any nonmechanistic system could not recognize the existence of cause-effect relationships. But parapsychologists, as well as other scientists, conceive of *all* the laws that they *recognize* as being expressions of inviolate cause-effect relationships in nature. Psi researchers are not mystics or animists, as Walker seems to assume. The distinguishing characteristic of the usual meaning of mechanism is a belief in the basic *unity* of all natural law within *one* system of integrated causality rather than an assertion that all events are governed by law. The latter assertion would be agreed on by both mechanists and nonmechanists. Most parapsychologists would not be mechanists, for they believe there are *two* such systems of integrated causality.

Walker has not made clear the real issue that Rhine seeks to raise, namely: Are there cause-effect relationships in nature that do not obey—indeed, may operate

counter to—the laws that govern the presently known mechanisms of nature? This means, is it all one world, or two? Are there two kinds of sets of mechanisms in nature, whose laws are different and contrasting rather than homogeneous and monistic? The view of most parapsychologists is obviously that there are two kinds of laws, although some researchers in the field have felt that ESP must somehow be governed by the same type of laws that applies to the rest of nature. Typical of the latter is the point of view of J. H. Rush, who logically enough is a physicist. Therefore Rush and some of the others in the parapsychological camp would properly be called mechanists under the usual definition of mechanism. Walker and the other so-called mechanists hold, like Rush, that there is only one kind of law, namely, the kind with whose workings scientists have already become widely familiar. In other words, the mechanists are physical monists, while Rhine and his school are psychophysical dualists.

Rhine has repeatedly stated that he is not a dualist. In personal correspondence with me he states that he is "no more a dualist in philosophy than anyone who differentiates between the eastern and western hemispheres, or between waves and particles in physics. . . . Everyone is a relative dualist or pluralist, since there is no one who does not differentiate between the various areas of operation in nature." But his insistence on psychic laws as apart from physical laws has led most interpreters of his position to conclude that the term *dualism* applies, and that his setting the term aside is equivocation.

Much confusion results from the use of the term *physical*. The physicists have tended, like Rush, to insist that if ESP exists it must have a physical basis. To them this term seems to cover anything that is real. To Rhine and his school the term refers only to those reals that obey the set of laws with which we are already familiar; there are other reals that obey another set of laws that may be called psychical. But Rhine and his followers definitely consider the "nonphysical" laws that are presumed to govern psi phenomena as expressions of causal regularity, and they further hold that these laws are the proper subject of scientific investigation with a view to prediction and control of events in nature, just as the laws that govern the known physical relationships are studied with a view to such prediction and control. The labeling of an occurrence as a psi phenomenon does not mean to a parapsychologist that Q.E.D. is written after the phenomenon as adequately explained, in the way that Walker erroneously supposes.

The crux of the battle is whether, after all, ESP and related phenomena (assuming that they occur) can be reduced, like all other aspects of behavior and experience, to mechanistic systems that obey known laws of matter and energy, or whether these phenomena require the postulation of an independent set of laws for their adequate explanation. If all can be reduced to one mechanistic system, nature is all one world (physical monism). If not, nature becomes two worlds (psychophysical dualism).

In order to aid in clearing the air of the semantic confusion that grows out of the fact that different meanings are often attached to such terms as *materialism, mechanism, vitalism, dualism, nonphysicalism,* and the like—as well as to the ESP hypothesis—the following usages are suggested as all that are basically needed to express the various meanings of these terms: (i) φ (*phi*) *systems of causality* as a term for matter and energy systems of a physical nature, as these have been traditionally understood through the known laws of matter and energy. (ii) ψ (*psi*) *systems of causality* as a term for matter and energy systems of a nonphysical form, that is, for reals that obey another set of laws that may be called psychical but may not obey—and may even run counter to—the known physical laws. Although these psi systems do not obey the laws of mass, energy, and so forth, of the phi systems, they nevertheless, in operating under their own laws, adhere to an inviolate causality. They are not haphazard or mystical.

It is conceivable that the term *mechanism* might be applied to either psi or phi systems. Similarly, the ESP hypothesis can be defined in terms of either psi or phi systems. For Rhine, ESP operates in a psi system; for Walker, it operates in a phi system. For Rhine, mechanism refers to variables (known or unknown) in phi systems only, whereas for Walker, it refers to variables (known or unknown) in either phi or psi systems —if psi systems indeed exist, as Walker does not seem to grant. All the confusion over the meaning of the term *mechanism* may be avoided, however, by simply referring to psi or phi systems while keeping in mind the definition of each.

JAMES C. CRUMBAUGH
Department of Philosophy and Psychology, Memphis State College, Memphis, Tenn.

Purity, Body, and Flavor: the Applied Scientist

As psychologists in a department that trains graduate students in several applied areas, we have students who ask questions about and express attitudes toward applied science. This has stimulated us to do some thinking about the applied scientist. The resulting analysis will probably not be acceptable to many of our colleagues, but here it is for what it is worth.

One still sees basic or pure science contrasted with applied science. In the recent past, Frank Geldard of the University of Virginia has discussed the differentia of basic and applied research [*Am. J. Psychol.* **66,** 335 (1953)]. He says, "I do not find one more scientific than the other. I do not find one characterized by trial and error procedures. I do not find one in the hand of selfless dispassionate fellows, the other controlled by self-seeking, avaricious people. The sole difference seems to be that the 'pure' researcher is at liberty to use any and all manipulative variables as independent variables which, when used as independent ones, do not change appreciably the situation from which they were extracted." Frederic Bartlett, the distinguished Cambridge psychologist, said recently that the only criterion of fruitful research is that while it answers some problems it leads on to others. Perhaps

it is an emphasis on *answering* problems rather than on *provoking* them that distinguishes applied and pure science.

In any event we believe that one useful way of looking at the problem is to consider the motives of each scientist. A scientist who perceives himself in the business of problem-solving is a scientist doing applied research. A scientist engaged in examining a set of events because he is interested in these events, per se, and is not concerned with the direct relevance of his studies to human affairs is doing basic research. It should be obvious, but apparently is not, that this differentiation is not so much a function of the methods or techniques used but of the motivational-perceptual system of the scientist. Basic research has had and will continue to have applied significance, without any such intention on the part of the scientist producing the research. The work of the applied scientist has yielded and will continue to yield basic scientific findings as a by-product of its problem-solving function.

The same individual can and does operate both as a pure and applied scientist. Such double-duty individuals occur frequently in psychological affairs. Despite the fact that most psychologists were trained as pure scientists before World War II, these same individuals are now performing successfully as applied scientists.

May we make one last suggestion? Within their own disciplines individual scientists appear to differ in their willingness to expose themselves to the research climates of systems that vary from simple to complex. Applied research tends frequently to be of the complex, multivariable type; it tends to be contaminated by uncontrolled sources of variability; it tends to be costly; and it frequently involves professional risk on the part of the investigator. Some scientists appear to seek out such research climates; others avoid them. A few scientists appear happy in either climate and shift from one to the other with obvious pleasure in both. These are idiosyncrasies of the scientific investigator.

This point of view may also be useful in examining the preferences of certain individuals for being identified with specific disciplines. It may be that a scientist selects for his efforts the science whose concepts and techniques represent a comfortable level of abstraction and error for him. Variable concerns, and variable flavors characterize the body scientific, and we applaud and encourage such variability as being in the best interests of the further development of the scientific enterprise.

SHERMAN ROSS
RAY C. HACKMAN
University of Maryland, College Park

The fact cannot be disputed that great discoveries regarding the behavior of the external world have been made by workers whose investigations in their field of research were not related in their own minds to any interest or belief outside it. But the effect of such segregated thinking has been to make science a departmental affair, having no influence on life and thought except indirectly through its applications. At the present time there is a movement in scientific circles aiming at securing for science a greater influence on human affairs, and even calling for a refounding of civilization on a scientific basis; but its advocates do not always understand that, as a necessary condition for the possibility of such a reform, science must be reintegrated into a unity with philosophy and religion.—E. T. WHITTAKER, *presidential address, Royal Society of Edinburgh, 1942; Science* **98**, *270 (1943).*